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OF THE

ROYAL METEOROLOGICAL SOCIETY

Editor: P. A. SHEPPARD

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OF THE

ROYAL METEOROLOGICAL SOCIETY

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EDITORIAL

Twenty years ago the Editor of the *Quarterly Journal* instituted a series of 16 articles under the title *Some Problems of Modern Meteorology** in which the writers attempted to assess the current state of knowledge in various branches of meteorology, and to formulate the major problems requiring solution. Those who remember the series or who know it from recent reading — for there is still much of relevance to be found in the articles — will agree that it went some way towards satisfying two kinds of reader : those who were active workers in the subjects discussed and for whom a stocktaking was thus provided, and those who wished to discover generally where meteorology was going.

The period since that series first appeared has been one of unprecedented development in the means, technical and financial, which have been applied to discovering what goes on in the atmosphere, largely of course through the routine and widespread use of the radio-sonde (often including radio wind) though other developments, in aircraft and rockets for example, are not to be overlooked. Now we cannot expect nor would we wish that the atmosphere should give up all its secrets — even *in principle* only — at the first simultaneously integrated and large-scale attack upon it ; indeed we know sufficient of our ignorance to realize that those secrets are not to be discovered by observing only the pressure, temperature, humidity and wind, on however large and intensive a scale. But the assault has now been pressed for a sufficient time that it seems worth while to step outside the battle for a moment and try to assess the value of the ground that has been gained and how that should affect our future strategy — to attempt in fact a re-formulation of our problems and a discussion of how best to work at them.

We are therefore beginning a new series of articles, of which the first appears in the present number, entitled *Reviews of Modern Meteorology*. In them authors will be given scope to survey *critically* recent progress in their fields, maybe to contrast the present position with that of a century ago — the series being born of the Centenary year of the Society — and then to guide our eyes towards the distant and significant view.

To ask a man to tell us what his problem is sounds a very simple question. But to answer it in a significant fashion is often very difficult. We might even go so far as to say that the importance of a problem is to be measured by the difficulty experienced in formulating it clearly, granted always of course a reasonable degree of intelligence and imagination in the person attempting the task. By the time that problems can be readily and clearly stated they are generally three-quarters

* Later published in book form by the Society under that title. Copies are still obtainable from the Society.

the way to being solved. Of what major meteorological problem can that be said — unless we state our problems in such general terms as " Why the weather ?" and show ourselves lacking in both the intelligence and imagination assumed above.

As often in the past in physics (and in human affairs too), we may even have been asking ourselves the wrong questions — is the problem of the general circulation such a one ? Moreover, there is a special difficulty in an observational science such as meteorology in deciding upon a reasonable starting point for our theories. What is this atmosphere whose behaviour we wish to understand and so to forecast ? Is it one of given chemical constitution and mean temperature distribution irradiated with a given insolation ? It has certainly seemed reasonable so to regard it for many, *e.g.* dynamical, purposes, though we know that effect reacts on cause and that, for example, many of the thermal properties we may assume are definitely the outcome in part of the dynamical processes we infer from our " given " atmosphere. In modern jargon, we deal with a system employing *feedback* — a " closed " system in fact for which there are no " initial conditions," other than those we observe at any moment. For example, the climate of the troposphere is in some sense determined by the amount of atomic oxygen at 100 km and *vice versa* though we probably do wisely at this stage of the subject not to pay much attention to such possible relations.

Thus, although the main aim of the series now beginning is easily stated the reader should perhaps be warned that the answers to be given number by number are not likely in all cases to be clear-cut, when probing mind and clear exposition have already, we assume, done their best for him. Indeed we wish this series to set reader as well as writer thinking, and if the first reading should be in the arm-chair — for we also assume entertainment value — we hope there may be some later readings in more business-like attitudes.

Meteorology is a branch of physical science, not a recreation ground for mathematicians *qua* mathematicians, (though some readers may perhaps wonder at this statement with the literature what it now is). It should then be possible to state the essential progress of the subject and the nature of its present problems in terms of physical ideas (or measurable quantities) — the way things *work* in fact — without detailed mathematical formulation or the use of rather intangible concepts. Thus the present series of articles with scope we hope befitting the present state of the subject, will not make use of mathematical argument unless clarity, conciseness and power of expression demand it. Physics can still look respectable without mathematical clothes though numbers are of its very being.

We remarked above that the most notable feature of meteorology during the last decade or so is the growth of knowledge of what goes on in the atmosphere, judged in terms of the readings of instruments and the more obvious conclusions which may be drawn from them (as shown for example by synoptic charts). It seems fitting therefore that the first of our series of reviews should be devoted to *Meteorological Measurements*. No doubt later articles too will have something to say about measurements, but, ours being an observational science, the principles and practice of measurement are so important as to justify themselves as a branch of the subject. By the time that the end of the series is reached we may hope to have seen to what good purpose the measurements have been and may be put

551.50

1. Meteorological measurements

By A. W. BREWER and F. J. SCRASE

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1. Introduction

The advance of any science depends upon the progress made in its experimental techniques, its observations and its measurements, for without sound observational data even the wildest theory is always right. Meteorology is not exempt from this rule. It is, in fact, more than usually dependent upon observations since its purpose is to describe, explain, and predict the nature of the earth's atmosphere, rather than to account for the results of a specially prescribed experiment. It is, therefore, particularly appropriate that a series of *Reviews of Modern Meteorology* should be opened with an account of the developments which have occurred in meteorological measurements.

Meteorological observations are generally used either for climatology, synoptic forecasting, or basic physical research, and the three branches are surprisingly separate, often making use of different types of observational data or using the same data mainly for convenience. The differences are partly due to the special needs of the three branches and partly to the different techniques which can be employed in the collection of data.

Climatology is the study of average weather conditions, largely with regard to their impact upon human affairs, including of course the animal and vegetable life upon which man is dependent. For climatological purposes direct measurement of the effective elements of the weather is usually made, and the averages are taken in a direct way. The observations must be made for long periods, often in isolated or remote regions, and simplicity and reliability are the main requirements for the observations and the observing equipment. In the traditional climatological observations, no large or rapid changes in practice are to be expected, nor are they particularly desirable and we shall have little occasion to refer to them further ; but today many new meteorological factors are becoming increasingly important in climatology, and in some cases progress is being hindered by lack of data. With the increasing importance of air transport, climatological data on upper air conditions for all parts of the globe are urgently needed. These include upper winds to great heights, gustiness, prevalence of icing conditions, and the conditions which affect an aircraft when landing, such as cloud amounts, cloud ceilings, and air to ground visibility. These measurements, now required to be a part of climatology, have already become an integral part of synoptic meteorology and they will be discussed later. The growth of observations of this kind has greatly increased the breadth and importance of physical climatology.

Atmospheric pollution is an important climatological study which was greatly

facilitated by the methods of measurement developed by J. S. Owens soon after the first world war. There have been no noteworthy new developments in recent years, but the measurements which have accumulated over the past 30 years have recently been discussed by Meetham (1950).

One climatological measurement which has not so far been satisfactorily achieved is that of the integrated ultra-violet (U.V.) light intensity. The U.V. light is of considerable biological importance since it is required for vitamin D synthesis in animals and human beings. Because of the lack of a suitable instrument it is not known to what extent conditions in large cities are deficient in U.V. light. Measurements are now being obtained, however, of daylight illumination in large cities.

For synoptic purposes the observations made are of those quantities which can both be readily measured, and which are known to be of immediate application to short-term forecasting. Reasonable simplicity is essential since a wide-spread network is necessary. There has been little recent change in the methods employed for surface observations ; such improvements as have been made are mainly in regard to details of instrument design, to the precision of the observations or to avoiding the limitations of older techniques. The observation of visibility at night without a good selection of illuminated visibility marks is a good example of the latter. In the field of upper air observations, however, there have been immense developments, and forecasters have been greatly exercised to make full use of all the upper air observations which they regularly receive. Also it is by the use of this new information that the existence of such features as the jet stream and the long waves in the upper air has been demonstrated, and their study made possible.

For routine meteorological observations there is a limit to the accuracy which can be usefully employed in most measurements — a point which has been fully discussed by Middleton (1946). Obviously there is no purpose in measuring a property of the atmosphere to very great precision when it is changing very rapidly or when it is different only a short distance away. For this reason the trend to ever higher accuracy of measurement, which is so desirable in many sciences, is not so important in most of the measurements of synoptic meteorology or climatology ; the trend is rather to simplify the observing equipment and to make it available for observations in ever more remote places, or for use by less skilled personnel. In these circumstances high accuracy is less important than reliability and stability.

It is in the field of physical research that there have been the greatest strides. In measurements for research purposes, the special requirements of instruments used for routine climatology or for synoptic observations do not, in general, apply. The field, too, is much wider as we are no longer restricted to observations which are of direct practical use. The study of physical meteorology is essentially long-term research, and many of the methods of measurement, unlike those for routine requirements, are more or less continuously under development, though they may have no immediate prospect of economic application. A considerable part of this review will, therefore, be devoted to the methods of observation and measurement which are being used in fundamental and applied research in meteorology.

2. Developments in routine meteorological measurements

(a) *Surface-pressure measurement*

The accuracy which can be profitably used in the measurement of surface pressure is limited to about 0·1 mb since uncertainties greater than this arise from the local effects of wind and in the reduction to sea level. An accuracy of about 0·1 mb is obtainable with the well known standard types of mercury barometer. Efforts to achieve this accuracy with the very much more convenient aneroid barometer have continued, but with only partial success, for although precision aneroid barometers can be made to give the required performance, they cannot be relied upon to retain it over long periods. Changes in the internal structure of the metal of the capsules cause changes in the calibration of the instruments, and frequent checks against a reliable barometer are necessary. A few countries have adopted the practice of using an aneroid barometer for the daily routine observations, together with a hypsometer for calibration purposes. The hypsometer measures the boiling point of water, which varies with atmospheric pressure, and it has long been used as a rough barometer. By using a very high grade mercury-in-glass thermometer to indicate the boiling point, measurement of pressure with an accuracy of the order of a tenth of a millibar is achieved, thereby making a very light weight and portable standard barometer. The arrangement is very convenient, but it has not yet come into wide use because of the difficulty of making the very accurate liquid-in-glass thermometer. The use of an electrical thermometer for this purpose opens up wider possibilities of application, for example in radio-sondes (see section 2(g)).

(b) *Measurement of surface wind*

The pressure-tube anemometer and the cup anemometer remain the standard instruments for wind speed measurements. Numerous improvements have been made in the design of the cup anemometer, the main changes being the introduction of conical cups with a suitable rim which improves the linearity of the relation between rate of rotation and wind speed, and also by the use of light alloys, a great reduction in the moment of inertia of the rotating system to reduce the effects of a gusty wind. Since the torque on the cups, if they are not rotating in equilibrium, increases approximately with the square of the wind speed, it follows that in a gusty wind the anemometer reads high. Reducing the moment of inertia of the cup system helps the cups to follow the wind and reduces this effect.

The most urgent need is for a satisfactory distant-recording anemometer sufficiently inexpensive for widespread use, particularly at synoptic stations. Such an instrument should provide for the measurement of mean wind and also of short-period gusts, such as are recorded by the Dines pressure-tube anemograph. In general, the need has been met by a cup anemometer driving a small electric generator, the output of which is fed into a recording voltmeter. The accuracy of such instruments, over a range of 5 to 100 kt, is usually much better than the average exposure of anemometers would justify. One drawback to this type of instrument is the inability of the cups to turn the generator at low speeds.

Wind speed measurements made for special purposes will be described later.

(c) *Measurement of visibility*

In an era of increasing use of high-speed transport the reporting and forecasting of visibility is becoming more important, and the problems of its proper measurement have received considerable attention. The solution of the problems has proved difficult because the definition of visibility was formerly very vague, its observation was entirely subjective, and several peculiar features of the methods of measuring, or observing the visibility have become accepted from long usage. In addition to its basically subjective nature, the method of making the observation, by the use of a series of visibility marks, automatically gave the average property of the atmosphere over the entire path between the observer and the furthest visible mark. Thus in good visibility it gave the average figure for a large distance, but in fog the visibility in the immediate vicinity of the observer was given. This sampling procedure is very convenient in use but it is difficult to reproduce instrumentally.

Not every station has a satisfactory range of visibility marks for use by day and night, and at sea the position is much worse, since in mid-ocean one does not find a good selection of suitably placed objects. By day, even with few visibility points, a competent observer can usually make a very good guess, but at night the position is quite unacceptable, and there is an urgent need for a visibility meter which is not dependent upon remote objects.

We are primarily interested in the visibility as given by the subjective observation, but until Koschmieder (1924) published his theory of vision through the atmosphere, meteorologists had no idea how to tackle the problem, or what physical property of the atmosphere was in fact being measured by the visibility. Koschmieder's theory is simple and attractive, so much so, that it has been surprisingly slow in gaining general acceptance. However, it is now accepted, and in order to permit interpretation on the basis of his theory, the meteorological visibility is defined as the greatest distance at which a dark object of reasonable angular subtense can be seen against the adjacent sky, and recognised for what it is. By Koschmieder's theory the distance is equal to $(ln\ 1/\epsilon)/\sigma$ where ϵ is the apparent optical contrast (usually taken to be 0·02) between the target and its background when the object is just visible and σ is the optical extinction coefficient of the air.

At night the visibility is defined as the visibility if it were daylight and atmospheric conditions were unchanged. This scale can be realised in practice by the use of visibility lights, if the brightness of the lights is suitably graded, using very small candle powers for short ranges and large candle powers for the distant lights.

It will be seen that Koschmieder's theory makes it possible to replace the subjective observation of visibility by some instrumental measurement of the extinction coefficient of the air. But it must be remembered that it is the subjective visibility which is required for most practical purposes. The theory also shows how the visibility can be derived from a measurement of the light scattered by the air between a dark object and the observer. There has been a great effort to produce suitable and simple instruments based on these principles. Some are described by W. E. K. Middleton (1941) and there is an excellent summary of the present position by G. J. W. Oddie (1950).

Visibility meters for use by day generally consist of an optical arrangement whereby light from an adjoining area of sky is superposed upon the image of a distant black object until the black object begins to disappear, or, as in Löhle's meter, where images of the sky and the black body are placed side by side in the field of view and the brightness of the sky is reduced until it equals the apparent brightness of the black object. These two methods are often classed together and are regarded as suitable for use in visibilities up to say five times the distance of the black body.

This is true of the first type, for if the body is close, and the air light between it and the observer is small, then almost the whole of the sky brightness must be added to the brightness of the black body to make it disappear. The disappearance point is ill defined, for the eye is not particularly sensitive to this. Also, since it is the brightness of the black body which ultimately gives the visibility, then small errors in the added sky brightness to achieve disappearance will give large proportionate errors in the black-body brightness and therefore in the visibility.

The principle of Löhle's instrument is more satisfactory in this respect. Löhle's instrument would fail in practice when a natural, dark, but not truly black object is used, because it would not be possible to distinguish between air light and the original brightness of the object.

Beuttell (Beuttell and Brewer 1949) made a similar instrument but avoided this fault by using a very carefully constructed artificial black body. He used a path length of only 2 m. Nevertheless visibilities of 50 km could be measured and even the scattering from pure air with visibility over 150 km could be detected. Under the best conditions of visual photometry two comparison fields can be adjusted to a precision of about 0·2 per cent. and in principle Beuttell's instrument should be capable of determining the attenuation coefficient and hence the visibility to this same accuracy. There is no reason why an instrument for use on ships should not be developed on these lines, but the limitations of a short sampling path must be borne in mind ; they can be partially overcome in a moving vessel or by repeated measurements.

The simplest instrument for use at night is the Gold visibility meter (Gold 1939) which consists essentially of a neutral optical wedge. A distant light is observed through the wedge and the opacity increased till the light is just visible. The additional opacity required to obscure the light is used to extrapolate the visibility from the known actual distance and brightness of the light. For most observers, particularly those with first class vision, the instrument is not easy to use since nature has seen to it that the eye is a most efficient light detector with a very variable threshold of sensitivity to suit the conditions of use. The absolute sensitivity of the eye varies over such a large range, depending on the state of adaptation and on the retinal position of the image, that to try to use the threshold of visibility as a datum, except possibly in very closely controlled conditions, is not a good principle of measurement. When a standard source is introduced for comparison, more satisfactory results are possible, but in that case the method loses its attraction of simplicity.

Various visibility meters have been developed which measure directly the attenuation of a parallel beam of light. This principle lends itself to obtaining

representative observations over reasonably long sampling paths and those instruments in which the measurements are made by means of a photo-electric cell are readily adaptable for continuous recording. There are, however, numerous practical difficulties due to misalignment, dirt on lenses and windows and to stray light which can lead to errors comparable with the attenuation itself, which may be small unless the path length is a significant fraction of the visibility.

Usually the attenuation of light in a parallel beam is almost entirely due to scattering and the attenuation can therefore be measured by measuring the scattering. Waldram (1945), in a brilliant wartime research which deserves to be more widely known, constructed a photometer which measured the light scattered by a small volume of air which was artificially illuminated. The photometer measured the polar curve of the scattering which was integrated graphically to give the total scattering coefficient. This was shown by control experiments to be substantially equal to the attenuation coefficient from which the visibility is given by Koschmieder's relation. The instrument was mounted in an aircraft, and atmospheric scatter curves were obtained on a number of occasions at levels up to 30,000 ft. The movement of the aircraft was used to pass outside air through the instrument and ensured proper sampling.

A similar arrangement has been used to make a ship's visibility meter known under the code name *Loofah*. In this instrument the scattering in one particular direction is measured. The direction is chosen carefully so that the measurement is a sufficiently good approximation to the integrated effect in all directions. This is not really very satisfactory for, as Waldram has shown, polar curves of scatter vary very markedly. This objection has been eliminated by Beuttell in two night visibility meters which were described by Beuttell and Brewer (1949). Beuttell measured the light scattered into the eye from a lamp placed on the side of the viewing track, or alternatively the light scattered on to a photo-cell from a parallel beam of light passing in front of the cell. In this way he made an excellent hand visibility meter. The sampling is of course very poor but is improved is the observations are made on air in movement. Beuttell's work is worthy of further development, and his death on a meteorological reconnaissance flight during the war was a severe loss.

For the purpose of landing aircraft, the oblique visibility along the approach track, rather than the horizontal visibility along the runway, is the critical factor. On occasions of shallow radiation fog, reports of horizontal visibility may be misleading and an aerodrome may be unnecessarily treated as unusable. Thus something more than the horizontal visibility is required. Two ways are open here : the simpler one is to supplement the measurement of horizontal visibility with the measurement of the height of the top of the fog, *e.g.* by using some type of nephelometer on a captive balloon to indicate when the fog top is reached. From the two measurements the oblique visibility along the angle of approach can be deduced provided the fog is homogeneous. The other method is to obtain the visibility, or extinction coefficient, along an oblique path by measuring the attenuation of light from a source projected to a suitable height, as for example, part of a searchlight beam. Work is proceeding on both these methods and the results are eagerly awaited.

(d) *Measurement of upper wind*

The method of measuring the speed and direction of the wind in the upper air almost invariably depends on the observation of the movement of a balloon ascending at a more or less uniform rate. For some 40 years or so, such observations have been made by optical theodolites, with which the bearing and elevation of the balloon can be accurately measured. These instruments were improved considerably in the inter-war period and are still widely used. The improvements have included such things as the fitting of auxiliary finder telescopes, enclosed scales, micrometer hand-wheels and built-in lighting. A German design incorporating semi-automatic plotting of the observations found considerable favour on the Continent and a very attractive feature of an Italian model was the elimination of the gear drive for the bearing and elevation movements by the substitution of a friction drive. Unfortunately many of the improvements have resulted in increased weight of the instruments over their simpler forerunners. Quite apart from their continued usefulness for routine observations, optical theodolites provide a convenient means for the independent checking of other methods of tracking balloons. The great limitation is, of course, that cloud and fog prohibit their use.

This limitation was first overcome a few years before the war by the use of radio direction-finding. In this system, which in this country was developed by the *National Physical Laboratory*, the balloon carried a small transmitter and its plan position was located by bearings indicated by three direction finders about 20 mi apart at the corners of a roughly equilateral triangle. This method enabled the balloon to be followed over long ranges and therefore to great heights and through cloud. At the radio frequencies used however, namely about 27 Mc/sec, it was liable to give appreciable errors due to ground reflections.

The radio direction-finding technique was carried a step further during the war by the development, in the U.S.A., of an equipment known as SCR658, with which both the bearing and the elevation of a balloon transmitter could be measured. The equipment could thus be described as a radio-theodolite, and knowing or assuming the height of the balloon, it enabled the wind to be determined from a single station instead of necessitating the costly three-station network. A frequency of 400 Mc/sec was used, the directional receiver aerial consisting of an array of dipoles arranged in four bays which could be rotated vertically and horizontally. The instrument has been described by Kirkman and Lebedda (1948) and an instrumental angular accuracy of 0·15° for elevations above 15° is claimed. The actual errors of observation were two or three times as large. At elevations lower than 15°, ground reflections introduce quite large errors. The same authors have described a new radio-theodolite known as AN/CRD-1, which has now been developed in order to overcome the shortcomings of the SCR 658. To reduce the ground reflection trouble, a radio frequency of 1725 Mc/sec is used. The aerial is a single dipole at the focus of a paraboloidal reflector, which is arranged to direct and maintain its beam on to the balloon automatically. The results of tests of this equipment have not been published, but they should compare favourably with the performance of high class optical theodolites. Like its predecessor, however, the new equipment relies on height being obtained from a radio-sonde.

In this country upper winds have, for the past seven years, been measured by

tracking balloons by radar, an Army gun-laying type of set working on a frequency of 3000 Mc/sec being used. One great advantage of radar is that it provides, in addition to the bearing and elevation, an accurate measurement of the slant range of the balloon and is, therefore, completely independent of pressure measurement for deriving height. Another advantage is that it dispenses with the balloon transmitter, a less costly reflector being used instead. The disadvantages are that it necessitates a high-powered ground transmitter, and at the present state of development its range is limited to about 50 mi.

The problem of accurate upper wind determination at much greater ranges is being pursued in this country by the use of a system of secondary radar. In this system the ground station, which may be called a radar-theodolite, transmits pulses which are returned at a wave length of 10 cm, corresponding to a frequency of 3000 Mc/sec, by a transponder carried by the balloon. The power of the ground transmitter need then only be a fraction of that required for the primary radar method, but a range of 100 mi is obtainable, and with automatic tracking and integration of readings by the ground receiver, it is hoped that a vector accuracy of 2·5 kt in wind velocity will be achieved.

The biggest drawback to the application of radar technique (and also radio methods which involve extremely high frequencies) to wind finding, is undoubtedly the complex nature of the equipment, and therefore its high cost both in initial outlay and in maintenance. Unfortunately there seems to be no way of avoiding this without sacrificing much in accuracy and range. No system which necessitates assuming the rate of ascent of the balloon is likely to be generally acceptable nowadays and neither is any system which involves more than one ground station. The choice is thus limited to methods in which :

(1) height is derived from radio-sonde pressure measurements and either range and bearing, or bearing and elevation are measured.

(2) range, bearing and elevation are measured by primary or secondary radar.

All these methods necessitate the measurement either of range or of elevation, and in order to achieve the present-day requirements of accuracy of, say, 10 m in range and 0·1° in elevation, one is forced to adopt either the radar technique or a radio-theodolite system working at a radar frequency. It is essential to avoid radio systems using frequencies at which ground reflections cause large errors. This means that frequencies higher than 600 Mc/sec must be used. At the other extreme, we are limited by the fact that at frequencies greater than 10,000 Mc/sec the attenuation of radio waves by cloud droplets becomes more and more appreciable. Whatever system is used on frequencies between these two limits, it is certain to involve relatively complex apparatus of high precision. There is one question which should be borne in mind in considering the alternative under (2) above. If a system which determines height by radar (primary or secondary) is combined with radio-sonde measurements, is it necessary to include both temperature and pressure indications on the radio-sonde? Ideally it is only necessary to observe two of the four main variables, height, pressure, temperature and density, since the other two can be derived from the hydrostatic equation ($dp = -g\rho dz$) and the equation for a perfect gas ($\rho = p/RT$). For example, in the high atmosphere density and height are often measured and the temperature structure is obtained by calculation.

In radio-sonde practice it might be an advantage to measure height and pressure and to derive temperature. The best procedure depends on which quantities can, in practice, conveniently be measured with the highest equivalent accuracy.

(e) *Radio-sonde measurements*

Upper air observations are now being made on a scale which few could have envisaged 20 years ago, when upper winds were only available from pilot balloon observations, and temperatures for synoptic use were obtained by aircraft ascents at a very few places, usually to no higher than 500 mb. Meteorograph data were obtainable to greater heights, but only on a very meagre scale and could not be made available in time to be of immediate use to the forecaster.

Today observations are being obtained regularly up to the 150 mb level at least, on a world-wide if rather non-uniform basis. The soundings are now made by radio-sonde techniques which often include wind finding. It is in this respect that meteorology has been able to take advantage of recent technical progress. Great ingenuity has been shown in the development of a variety of types of radio-sondes and the stage of a standard design for general use has not been reached.

The various types of radio-sonde now in common use differ mainly in the way in which the meteorological measuring units control the radio signal. In one form, such as the Finnish sonde designed by Vaisala (1937), the meteorological units vary the radio-frequency by moving the plates of condensers in the single valve oscillatory circuit of the transmitter, the different units for pressure, temperature and humidity being switched into the circuit in turn by a wind-driven switch. This sonde has the advantage of being one of the simplest and lightest. Unfortunately the variable radio-frequency makes it inconvenient for use in regions where frequency bands are in great demand and it is also unsuitable for direction finding on the transmitter.

These particular disadvantages are overcome, at some sacrifice of lightness, in the types of sonde in which the meteorological elements modulate the audio-frequency of the transmitter. This principle is used in the British Meteorological Office radio-sonde, of which a full description has been given by Dymond (1947), and in the type in use in the U.S.A. In the former the audio-frequency is determined by the size of the gap in the mumetal core of an inductance which is controlled by the meteorological element. A disadvantage of this system, at any rate so far as pressure measurement is concerned, is the appreciable temperature coefficient of the inductance and its variability with the treatment of the mumetal. In the American sonde the audio-modulation is effected by variable resistances. Except in the case of pressure, this enables mechanical links between the meteorological elements and the electrical circuit to be eliminated. Both the temperature and the humidity units are in the form of resistors, but the pressure unit involves a mechanical link, and, therefore, probably some friction, between the aneroid capsule and a wire-wound resistor or *Baroswitch*. The latter allows the signals of temperature and humidity to be transmitted at predetermined pressures. The resistance method of audio-modulation has the merit of being simpler and lighter than the inductance method, but it is believed that, as used at present, the latter is rather more accurate.

Another class of radio-sonde is that known as the chronometric type such as is used in France and Canada. In this, the meteorological units actuate pointers which make contact with a spiral ridge on a rotating scanning disc or drum. The time interval between these variable contacts and a fixed reference contact is then a measure of the pointer deflection and therefore of the meteorological quantity concerned and can be recorded by a chronograph receiver. A variation of this type of sonde designed by Lugeon (1948) avoids the disadvantage of the mechanical contacts by using the momentary changes in capacity which occur when the spiral ridge passes under the pointers. The potential accuracy of the chronometric sonde depends mainly on the short-term constancy of the speed of rotation of the scanning unit. Thus to obtain a sensitivity of, say, 2 mb in a range of 1000 mb a constancy of speed to 1 part in 500 is necessary, and if the complete scanning cycle is to be less than 20 sec, the time of the pressure contact must be obtained to an accuracy of 0·04 sec. This is difficult to achieve with any simple inexpensive electric or clockwork drive, but the difficulty can be met by arranging for the motor to transmit timing pulses.

A somewhat similar principle is used in the code-sending sonde in which the pointers pass over a series of contacts so arranged as to provide a different morse code signal according to the position of the pointers. The disadvantage of this system is that the indications are discontinuous.

It is evident that since so many variations in radio-sonde design are still in use, some considerable time must elapse before anything approaching finality or uniformity in design will be reached. The trend of future design, however, will probably be to eliminate all mechanical links in the telemetering devices and to use, as far as possible, meteorological elements which can produce direct variations in some electrical quantity. The aim will also be to avoid the need for individual calibration of radio-sondes and this implies that components must possess good stability. It seems very unlikely that any really satisfactory method of measuring humidity by radio-sonde will be achieved in the near future and it might well be more profitable to concentrate efforts on devising a reliable method of indicating cloud boundaries.

(f) *High-altitude balloon observations*

Considerable attention has been given since the end of the war to the improvement of sounding balloons with a view to extending the height of radio-sonde ascents to well above the 18 km or so which has hitherto been regarded as satisfactory for routine ascents. The results of a number of soundings to 30 km and higher in this country and in U.S.A. have been reported, in the one case by Scrase (1949) and in the other by Brasefield (1950). Both series indicate much the same temperature and wind structure in the high levels, *viz.*, a rise in temperature with height of about 0·5°C per 1000 ft, with west winds in winter and east winds in summer.

There are of course, a number of difficulties associated with these high-altitude soundings, which necessitate some special instrumental developments. The pressures of the order of 10 mb encountered at 30 km are beyond the reliable range of the ordinary radio-sonde aneroid, and at these heights the air density is too low

r wind-driven switching devices to operate. Also, radiation errors increase pidly at great heights ; these are discussed in the next section.

(g) *The errors of radio-sonde data*

In using radio-sonde data in synoptic meteorology or climatology, it must be orne in mind that not only are they subject to casual observational and instrumental rors and to systematic instrumental errors, but also to deviations due to unrepre-ntative sampling Sampling errors arise because conditions in the upper air are ot entirely uniform, nor do they vary smoothly from place to place, but on account f eddies and discontinuities of every size, random variations are superposed upon he main features. The variations, though significant in the entire field of eteorology, are to be regarded as random when considered from the synoptic spect. For synoptic purposes the ideal would be, for example, to send up many alloons over a period of say 10-15 min and use only the mean data. This would e equivalent to taking the mean wind over a period of about 10 min, as is properly one in surface observations.

It is practicable to make one ascent only at each observation hour and the ack must be left to chance according to the eddies which determine it. The ampling errors which can thereby arise will be very variable and may sometimes e large. Durst (1948) has analysed observations of the fine structure of wind t heights of 5,000 and 20,000 ft by means of smoke puffs released at the appropriate eight and followed in a camera obscura. The puffs were released at short intervals uring a period of an hour. He found that there are short-period fluctuations in he wind of the free air, the periods of which are 5-15 min. In the cases discussed, he *vector* standard deviation of the fluctuations ranged from 0·5 to 3·5 kt. (Errors n wind measurement are usually expressed as *vector* errors ; the vector error being the vector difference between a measured wind and the true wind. Thus the measured speed may be correct but direction may be wrong ; by taking the vector ifference the error in direction is included). It seems probable that an instru-nental vector accuracy better than 1 kt is unnecessary. This has certainly not et been achieved, vector uncertainties greater than 5 kt being encountered specially on occasions of strong wind when the balloon is carried far away from he ground stations. Apart from possible errors in the heights, if these are derived rom radio-sonde measurements of pressure and temperature, the instrumental rrors which arise in wind finding are not of a meteorological nature and will not e discussed further here.

The useful precision for temperature and humidity measurements can also e determined. Satisfactory air-temperature thermometers and hygrometers are ow available for use from an aircraft, and thus with the help of an air-position-ndicator the moderate-scale temperature and humidity patterns of the size which would have a significant effect in a radio-sonde observation can be readily observed nd plotted. Frith (1948) has published isopleth charts of horizontal and vertical reas showing how variations in air temperature of the order of 1°C occur in patterns f size of 15-50 km ; the dew or frost point varies similarly but over a much greater ange, usually about 10 times larger. Thus there is little purpose in striving after

an instrumental accuracy of air temperature better than about 0·25°C (0·5°F). Even 0·5°C (1°F) would be quite tolerable provided it is not entirely systematic. In the case of humidity, an error in the dew or frost point of even as much as 5°C would be tolerable, but this is a difficult criterion to transcribe into terms of relative humidity, which most radio-sondes measure. Between 50 per cent. and 100 per cent. relative humidity, an error of ± 20 per cent. would be quite reasonable* but at low relative humidities the accuracy should improve proportionately, so that, at 5 per cent. relative humidity the error should not exceed ± 2 per cent. and at 0·5 per cent. it should not exceed ± 0·2 per cent. (Relative humidities of 2 per cent. or less are not uncommon in the upper air). This criterion is in no way met by most types of radio-sonde hygrometer which usually have a roughly constant error in the relative humidity, whatever its value, and usually work better at the high humidities. This is not appropriate for the upper air where relative humidities are very variable, and very low humidities, indicating great subsidence, have significance.

For the measurement of upper-air temperature the bimetal strip, the movement of which is converted into a quantity which can be telemetered, is by far the commonest sensitive element. The principal alternative is the thermistor. Both suffer to some extent from lag and from sensitivity to the effects of radiation, mainly direct sunshine. A lag coefficient averaging about 10 sec in the troposphere is usually achieved and this causes an error of about 0·3°C in a lapse rate of 6°C/km when the balloon is rising at the usual rate. At sharp inversions, where the temperature may rise at an equivalent rate of 40°C/km or even higher, considerable errors can arise, though it is very doubtful if a slight smoothing of the temperature curve at an inversion matters very much in synoptic work. Thus current lag coefficients are not unreasonable but might be reduced somewhat with profit.

The position with regard to radiation errors is less satisfactory. At low levels where the air density is high and the radiation intensity reduced, there is no significant error, but in the stratosphere the errors may become large. Attempts to eliminate the errors by radiation shields are not entirely successful for no shield can completely shut off all the radiation without slowing up the rate of ventilation of the thermometer or even heating up the air before it reaches the latter. The best way to reduce the radiation error to negligible proportions is to use a thermometer element which is small enough and sufficiently reflective to absorb only a negligible amount of radiation when fully exposed. Brasefield (1948) has described a white-painted thermistor which appears to approach this ideal. Most of the upper air data so far published however show a marked diurnal variation in the stratosphere and more tests are required before one can be certain how much of this is a real temperature variation and how much is due to radiation error. Kay (1949) has recently shown that the absorption of solar energy which occurs in the upper atmosphere is not sufficient to account for all the variation observed in this country. Diurnal temperature variation could however originate from dynamic causes. Fortunately for forecasting purposes, systematic errors such as those due to radiation and lag are not as troublesome as the large casual errors which inevitably

* This assumes that the hygrometer will not, in any case, indicate cloud boundaries. To do this it would need to have very high precision at 100 per cent. relative humidity. Most hygrometers have increasing lag at any temperature in the region between about 95 per cent. and 100 per cent. relative humidity.

ccur with the best equipment and observers. The trouble then is aggravated by he fact that the forecaster usually has no certain evidence that a suspected obser- ation is faulty.

Attempts have been made to measure the air temperature by measuring the elocity of sound at the level of the sonde ; this would avoid any physical element isceptible to sunshine. The practical difficulties are very large, and no successful scents have yet been described. E. Schopper has also suggested that the density ould be measured by measuring the β-ray absorption ; combined with the pressure his would give the temperature.

For the measurement of atmospheric pressure most radio-sondes use an aneroid apsule as the pressure-sensitive element, the mechanical movements of the capsule eing converted to a signal suitable for transmission. Near the ground where ne rate of change of pressure with height is large, the arrangement is very satis- uctory, but at high levels this element is less suitable, partly because the error n height due to small pressure errors becomes so large, and partly because of the irge temperature coefficient which necessitates corrections unless a satisfactory ompensating device is used. In the British radio-sonde the correction must also ake account of the temperature coefficient of the inductance telemetering unit. he probable error of a pressure measurement with this instrument at 30 mb *a.* 80,000 ft) is believed to be $\pm$ 5 mb which corresponds to a height error of 000 ft. The only alternative to the aneroid which seems to be possible at present a hypsometer. This has several advantages. Firstly, the roughly logarithmic ariation of vapour pressure with temperature matches the variation of atmospheric ressure with height and for a given temperature error in the boiling-point neasurement there is an almost constant error in the calculated height, the accuracy ncreasing at low pressures. Secondly, the quantity to be telemetered is a tempera- ure and by means of a thermistor or resistance thermometer this can be reduced o an electrical quantity directly, without the use of mechanical intermediaries. he hypsometer has the further advantage that the pressure calibration of the onde can be reduced to a temperature calibration, which may be a great convenience. Water could be used as the working substance, but there are many alternatives, hainly organic compounds, including the various freons. One possible difficulty n operation is that with a widely varying rate of ascent there might be a risk of ne hygrometer liquid either "going off the boil" or becoming super-heated. A ypsometer was used in a special radio-sonde ascent reported by Barrett, Herndon nd Carter (1949) and a design for an instrument of this type has been described riefly by Schulze (1950).

The measurement of humidity is the most unsatisfactory feature of all radio- ondes for the reason that all ordinary hygrometers fail at low temperatures owing o the excessive lag at the very small vapour pressures. This is a well known ffect which has always been a troublesome feature of low-temperature meteoro- ogical measurements. It may be mentioned that although at low temperatures he vapour pressure is too low to affect the air density or to make the wet and dry diabatic lapse rates significantly different, water vapour can nevertheless still play n important part in radiation, either as vapour or by forming tenuous clouds which adiate in spite of their thinness. Moreover the humidity still varies according to

the previous history of the air. Thus the humidity is of interest at all levels and should be measured if possible. For use on multi-seat aircraft, particularly for research purposes, this difficult problem has been solved by the development of the dew- or frost-point hygrometer, which will be described later, but there is nothing at all suitable for routine use on radio-sondes. The methods currently used, such as the electric absorption hygrometer, and the hair, or gold beater's skin, all fail in substantially the same way. At low temperatures the time taken for the element to adjust itself to a change in relative humidity is too large. The traditional wet-and-dry-bulb psychrometer fails because the temperature depression for zero relative humidity becomes negligible, quite apart from troubles due to the freezing of the wick.

One of the difficulties in assessing the accuracy of radio-sondes is the lack of any absolute standard of comparison which can conveniently be used in a balloon sounding. Something can be done by measurement from an aircraft flying round an ascending sonde, but sampling variations are likely to cause scatter in the results comparable with the errors and a good many comparisons are required to give reliable results. The only alternative at present is to obtain comparative data from pairs of radio-sondes ascending on the same balloon. Trials of this nature using several different types of sonde were recently organised by the *International Meteorological Organisation* and carried out in Switzerland. The results should provide useful information ; they are not yet available but a brief account of the trials has been given by Painter (1950).

(h) *Upper-air measurements from aircraft*

The use of aircraft for the measurement of upper-air temperature and humidity has become more restricted since the use of radio-sondes has increased. Nevertheless some important developments have taken place in the technique of meteorological measurements from aircraft, as a result of work carried out in this country by the *Meteorological Research Flight* of the *Meteorological Office*. For air temperature measurement an electrical resistance thermometer employing a manually balanced Wheatstone's bridge has been developed (Brewer 1947). This permits the thermometry to be carried out to the required accuracy. One difficulty in this work was the problem of the correction to the observed temperature which must be applied because of the high speed of the modern aircraft. In spite of pioneer theoretical work by Pohlhausen (1921) and experimental work by Hilton (1938), meteorologists were very slow to appreciate this problem. If the air speed of the aircraft is V the correction is in the form $\propto V^2/2Jc_p$ where $\propto$ is the correction coefficient and J, c_p have their usual significance. For the flat-plate type of thermometer element now used, careful trials were made to determine the speed correction and the corrected reading should normally be correct to within 0·5°C. Some doubt has recently been thrown on this by the suggestion that the correction coefficient, previously believed to be constant for the element, varies with altitude. The variation however is small, and on piston-engined aircraft it is difficult to prove the existence of the effect conclusively. So far no jet aircraft have been used for meteorological purposes in this country. Greater operating heights are now

ossible in jet aircraft, and it is hoped that they will soon be used for high altitude eteorological research. The problem of the speed corrections will then, once ore, become serious.

The flat-plate thermometer element was designed for routine use and for casions when the air-temperature measurement was an auxiliary observation. was used by Frith to observe the large-scale temperature fluctuations referred above, but its lag (*ca.* 5 sec) is such that it smooths out the very small scale riations. To study these, a resistance thermometer in which ·001 in. diameter atinum wire is exposed directly to the airstream has been made at the *Meteorological esearch Flight*. On an aircraft flying at 200kt the response time is about millisec, which means that fluctuations with a spatial scale of 30 cm would be produced. Indication and recording is by electronic techniques on to a cathode y tube which is photographed for subsequent analysis. Results obtained in this ay will be of great interest, particularly if they can be combined with measurements the vertical airflow.

For the measurement of humidity the development of the frost point hygro-eter to measure frost points down to —80°C (—112°F) represents a major advance. his instrument has been described in detail by Brewer, Cwilong and Dobson 1948). Air is piped into the aircraft from a forward-facing scoop and passed er a cooled metal surface so that a deposit of dew or hoar frost forms on it, the posit being obliquely illuminated and viewed through a simple magnifying lens. he temperature of the surface is adjusted until two temperatures are found at hich the deposit is (a) just growing and (b) evaporating slowly. The frost point the mean of the two temperatures. A skilled observer is required but Frith nnpublished) has shown that two aircraft flying side by side obtain substantially entical results. The basic accuracy of the instrument can readily be demonstrated a frost point of —78·5°C, (the sublimation temperature of CO_2) and this has een done by Brewer, Cwilong and Dobson (*loc. cit.*). The results obtained have een discussed by Dobson, Brewer and Cwilong (1946). The atmosphere is shown have a complex and highly laminar structure with details not shown by temperature easurements. Also, over South England the average frost point 1-2 km above e tropopause is found to be —76°C (—105°F) (Shellard 1949), corresponding a relative humidity of only 2 per cent.

A frost-point hygrometer has been operated on a radio-sonde by Barrett, Herndon nd Carter (1949), who have published the results of an ascent to 30 km. Apart om a moist layer about 300 m thick at about 16 km the results are in accordance ith results obtained from an aircraft.

A further development of the frost-point hygrometer has been the application a photo-cell method of detecting the frost deposit. This should facilitate the tomatic recording of frost-point and enable measurements to be obtained in ngle-seater high-altitude aircraft.

The need for a hygrometer which will operate at low temperatures and low solute humidities has been felt for a long time, and other solutions besides the w or frost-point hygrometer have often been suggested, either for use on the ound or in the air. One of these is a cloud chamber hygrometer in which the r is drawn into a chamber and then expanded, using progressively increasing

expansions, until a cloud is seen to form. Cloud will appear when the lowest temperature in the expansion is below the *dew point* (not the frost point, see the section on cloud physics below). If the initial temperature and the expansion necessary to form cloud are known the dew point can be readily calculated. The possibilities of this type of hygrometer have been briefly investigated by Palmer (unpublished), who finds that an easily visible cloud is obtained with dew or frost points down to about —40°C. Below that temperature the cloud becomes increasingly faint and without elaborate apparatus it is not detectable at —70°C. The range of the instrument is thus less than that of the frost-point hygrometer, which will work down to —80°C, but it has the advantage of avoiding the use of an external coolant. This should make it valuable for surface observations at low temperatures. In very clean air it is necessary to supply condensation nuclei, but this offers no difficulties.

(i) *Thunderstorm location*

For some long time the meteorological organisations in many countries, notably France, South Africa, U.S.A., Germany, Switzerland and Great Britain, have been conducting researches into methods of locating lightning flashes by direction finding on the radio noise which they produce. In England direction finding is carried out on a radio frequency of about 10 kc/sec (30,000 m wave length) from four stations, one each in N. Ireland, Scotland, SW. England and SE. England respectively. Two directional aerials are employed at each receiving station, one facing E.-W. the other N. S. and the signals which are received are amplified and recombined in a cathode-ray tube so that when a lightning flash occurs a line appears on the screen which indicates the direction of the flash. The flash is then located by combining the results of the four stations. The range of the system is about 2000 km. The results are useful to the forecaster, particularly for the location of cold fronts, occlusions and thunderstorm areas over the sea where the observing network is inadequate. For technical details see Adcock and Clarke (1947).

The great disadvantage of this system is the need for the widespread network of stations. The successful development of a single-station method in which, in addition to the direction of a flash, a reliable measure of the distance is obtained, is eagerly awaited. It has been claimed that this can be done by observing the intensity of the received signal, and assuming that the intensity at the source does not vary appreciably from one flash to another. It is well known however that the strength of lightning flashes varies over quite a wide range. The most promising, though difficult, line of investigation is the variation of wave form with distance.

3. Measurements made in meteorological research

For the purposes of research into the physical processes of the atmosphere a very wide and varied range of observational programmes has been reported or is known to be in progress and special attention is being given to the high atmosphere, the stratosphere and above. Although the atmosphere at these levels has little known influence on day-to-day weather, nevertheless it offers physical problems and conditions of special interest and difficulty. We shall, therefore, refer to these problems in some detail.

(a) *Radiation*

The various types of radiation measurements which can be made fall into two asses. There are those which are made for determining directly the thermo-ynamic conditions in the atmosphere, whereby its temperature and movements e affected or controlled, and those in which the radiation is used as a probe to vestigate the structure as it is found.

For the thermodynamic investigations what is primarily required to be known the divergence of the *total* radiation flux at every level in the atmosphere. Where e divergence is zero the atmosphere is in radiative equilibrium. Where there a net outflow from each elementary volume of air there will be a thermodynamic nk of heat and where the net flow is inward there will be a thermodynamic source. is by the transport of air from source to sink that the energy of all winds and orms is formed. This surely is the very foundation of meteorology. That the nportance of this has been recognised is shown by the large number of theoretical apers on the radiation balance of the atmosphere, which have followed each other roughout the last fifty years. They have the rather common feature of producing e desired results even though the initial assumptions vary greatly and have generally en subsequently proved wrong. There have, however, been few measurements the divergence of radiation flux.

Measurements of the long-wave, infra-red radiation at the ground were made W. H. Dines (1920) and similar measurements have been made at many ob-rvatories. Robinson's (1950) measurements in the lowest metre and Angström's 928) measurements in a manned balloon to 4 km provide the only information the divergence of long-wave radiation, and hence the net radiative cooling or ating. Goody (1950) has discussed the experimental difficulties.

Theoretical calculations of the infra-red radiation effects must be based on perimental determinations of the infra-red absorption spectra of water vapour, rbon dioxide and ozone, the main absorbing constituents of the atmosphere. enerally some theoretical interpretation, interpolation or extrapolation is involved. he importance and precise mechanism of temperature and pressure broadening the lines is still not known, and so far experimental observations of the absorptions columns of moist air, such as those of Brooks (1941), Elsasser (1941), and Robinson 950) have been made under conditions which do not realistically cover the wide nge of temperature, total pressure, and partial vapour pressures found in the mosphere. It is understood that Strong in the U.S.A. is hoping to obtain data the kind required.

There has recently been a great revival of interest in quantitative infra-red ectrometry and many new infra-red detectors, such as the thermistor bolometer, e photo-conductive cell, and the Golay cell, have been developed. We look rward to much progress in this field in the near future.

The short-wave radiation stream from the sun has been examined in many ays. Any possibility of the presence of day-to-day variations in the total intensity sunlight incident on the earth's atmosphere has been carefully investigated by bbot (1939) but as observations have improved, the magnitude of the apparent riations has tended to decrease, and the general view today is that short-period

variations are very slight. A recent study of the radiation incident on the ground at Kew has been made by Stagg (1950).

The depletion of the downward beam of sunshine is of importance in the same way that the variations of the long-wave fluxes are important, for it plays its part in contributing to the heating of the atmosphere by radiation. But it has also proved an invaluable probe for the study of the upper air, since in its path from the sun the radiation must pass through the whole atmosphere, each part of which can absorb radiation according to its composition. Even in the further infra-red, where the energy level is too low to be of direct thermodynamic significance, spectro-heliograms are providing valuable evidence on the absorptive and therefore emissive properties of atmospheric gases in those wave-lengths (see *e.g.* Adel (1950) and next section).

(b) *Ozone measurements*

It has long been known that the absorption of the far ultra violet ($\lambda < 2{,}000$ Å) is due to oxygen and occurs in the highest layers of the atmosphere, and that for wavelengths between 2,200 Å and 2,900 Å the energy is absorbed by ozone in the upper stratosphere (ozonosphere). The precise cut-off varies with the amount of ozone since the absorption in the region of 2,900 A falls gradually. Spectrographic observations of the cut-off permit measurement of the quantity of ozone. Dobson first showed that there was a day-to-day variation, a seasonal variation and also a latitude variation of the total amount of ozone. There is more ozone in high latitudes than in low, and at high latitudes there is a maximum in spring. For this work a photographic spectro-photometer was used. The photographic instrument has now been superseded by a most elegant photocell technique (Dobson 1931) and more refined measurements have shown the nature of the variations and the connection between the variations and the passage of fronts (Dobson, Brewer and Cwilong 1945). The instrumental improvements have also permitted observations on the zenith skylight. By following the changes which occur in the spectrum of the skylight, it is possible to obtain information about the height of the ozone layer, mainly the height of its *centre of gravity*, but possibly also the nature of the distribution curve. A further material improvement. has now been obtained by the use of photo-multipliers in which the photoelectric current is amplified by multiple secondary emission processes. The sensitivity of the instrument is thereby increased about fiftyfold. Observations are now possible on the moon and it has been possible to show that there is no change in the ozone during the night other than would be expected from the passage of fronts *etc.*; moreover, the zenith sky measurements are now so accurate that the mathematical interpretation of the results has become a serious problem.

Considerable importance is now attached to the measurement of the vertical distribution of ozone in the hope that it will provide clues concerning the formation of the ozone and of any movements which carry it from the region of formation. E. Regener (1934) made a small photographic spectroscope which was carried up in a balloon, taking spectrographs at intervals during the ascent. His results showed an extension of the U.V. spectrum at great heights and gave a vertical distribution curve for the ozone. Similar measurements were made from the

manned balloon Explorer II (O'Brien 1936). Recently further vertical distribution curves have been obtained by Durand, Oberly and Tousey (1949) whose photographic spectrophotometer was carried aloft by a V2 type of rocket. A rocket can reach great heights and a photograph was obtained at 55 km of the solar spectrum showing substantially no ozone absorption at all. The spectrometer used a fused bead of lithium fluoride, 2 mm diameter, in place of the conventional slit. The very small image of the sun which it formed made a point source which moved very little as the rocket turned. By this elegant trick the use of a heliostat was avoided. On at least one occasion the vertical distribution curve has shown two maxima of ozone density. The cause of this effect, if real, will almost certainly prove significant.

The general pressure level of the ozone has also been measured by Strong (1941) who used a very different technique. He measured the ozone content by measuring simultaneously the U.V. absorption (the usual technique) and also the infra-red absorption in the 9·7μ band in which he has shown that the absorption varies as the fourth root of the total pressure. From the two measurements he was able to calculate the pressure at the average level of the ozone, and thus its height.

We may also mention here that any ozone found in surface air is almost certainly that which has been transferred downwards by mixing and advection from the very high levels at which it is formed, and measurements of surface ozone concentrations are therefore a measure of these effects, which are of considerable importance. The ozone is usually measured by chemical methods and results have been obtained by Glückauf (1944) and by Emert and E. Regener. One of the main difficulties is to distinguish chemically between ozone and other oxydizing agents such as hydrogen peroxide.

(c) *Infra-red absorption measurements*

Infra-red absorption techniques have been used by Sutherland (1949) to demonstrate the presence of nitrogen dioxide in the lower atmosphere, and are in active use by Sutherland and Goody (Goody 1950) to measure the water content of the upper air by measuring the intensity of the solar spectrum in the region of the 9·3μ absorption band, from an aircraft flying at 10 km or higher. Because of the very large amounts of water vapour in the lower atmosphere it is necessary for the observing aircraft to fly in the stratosphere in very dry air, as otherwise the small quantities in the high atmosphere which it is wished to measure would be an immeasurably small addition to the total amount above the aircraft. The instrumental problems are severe, particularly as at great heights aircraft become dynamically unstable, and steady flight is very difficult. The variation of the absorption with height, when combined with independent frost-point measurements will also provide very valuable and realistic water-vapour absorption data.

(d) *Measurement of properties of the high atmosphere*

By many kinds of probes and observations our knowledge of the high atmosphere is rapidly increasing and the temperature-height curve is now generally believed

to be roughly that shown in Fig. 1. Measurement by balloon is limited to about 40 km ; above that height the methods of observation are mainly indirect. Attention was first drawn to the region of high temperature between 30 km and 60 km by Lindemann (now Lord Cherwell) and Dobson (1923) who showed its presence by observations on meteors. F. L. Whipple has continued this approach and has more recently (Whipple 1943) prepared a comprehensive summary of the whole subject. As in many methods of high atmosphere measurement the temperature is not given directly. The observations give the density and it is from the variation of this with height that the temperature is calculated. Whipple considered (1) the deceleration, (2) the altitudes of the ends of the track and (3) the point of maximum brightness of meteors, and by the measurements on these he was able to derive the probable density distribution. He obtained results for levels as high as 120km and his figures show the low temperature region at about 80 km.

This same region of the atmosphere has been investigated by the use of sound waves. The velocity of sound in air increases with increasing temperature and consequently the sound waves from explosions are bent down by the region of increasing temperature, producing zones of audibility far beyond the range of the ground wave. This was first noticed in the first world war. It was from the fact that the distant zone of audibility occurred to the east in the winter and to the west in the summer that F. J. W. Whipple (1935) concluded that there was a monsoon effect, with an east wind in the summer and a west wind in the winter, at the levels where the waves were bent down. He initiated experiments on the audibility of explosions at Woolwich using sound-ranging recording gear at six places

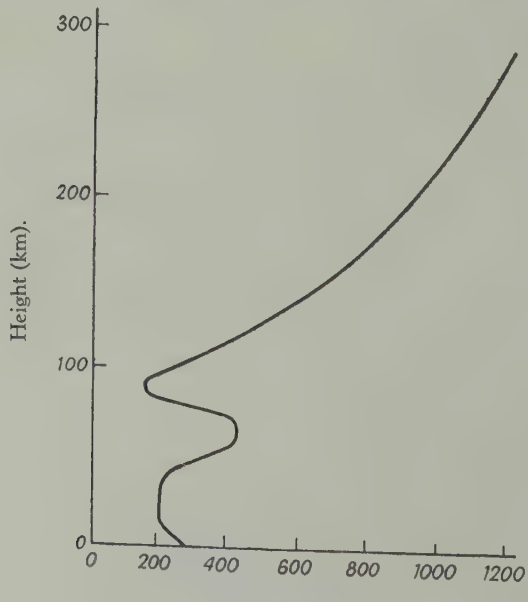

Figure 1. Variation of atmospheric temperature with height.

in England. The recording gear at each station included several microphones suitably placed so that the direction from which the sound arrived could be determined. The interest is, of course, in the slope of the sound path rather than its horizontal direction. From the measurements a probable temperature distribution could be drawn. More recently the experiments have been repeated in England but the results have not yet been published. Similar experiments have also been conducted in the U.S.A. where the procedure has been rather different. (Crary 1950). Only one observing station was used and the observations on travel in different directions were obtained by dropping suitable bombs from aircraft at the appropriate places. Sites for the observing station were chosen so that the bombs could be dropped over the sea or over uninhabited country. In reducing the results the effect of the upper wind is a serious disturbing factor which it is difficult to eliminate. In the recent U.S. experiment this was attempted by finding the direction of fastest travel of the sound, which is presumed to be the direction where the sound had a following wind. In a direction at right angles to this the effect of the wind should be much reduced and data obtained in this direction were used to calculate the temperature structure.

The existence of the region of falling temperature between 60 and 80 km can also be inferred from the limited audibility of such meteor explosions as occur above 80 km. Sound waves can travel directly downward through this layer, but if they are oblique they are reflected back into the upper air, and a distant observer cannot hear the sound. The explosion of the meteor can therefore be seen for a much greater distance than it can be heard.

We may also add that direct observations of the monsoon wind over South England have been made by Murgatroyd and Clews (1949) who followed shell bursts at 100,000 ft (27 km) from hyper-velocity guns. As already mentioned the occurrence of these winds has also been confirmed by high-altitude balloon soundings.

It is, in principle, possible to measure the variation of density of the air up to a level of at least 60 km, and possibly higher, by measuring the scattering of the light from the sun after sunset, or from a powerful searchlight, by the air at those levels. The scattering is in the Rayleigh range and so is well understood and measurements have been attempted by several workers. The experimental difficulties are very great but the steady development of appropriate types of high-power light sources and modulation techniques which has occurred during the last decade should now make useful results by searchlight methods possible. It is understood that J. R. Bauer has recently obtained results of density measurements to 61 km in Alaska using a modulated light source.

If new measurements of the properties of the high atmosphere are to be of use they must be of greater precision than those obtained by such methods as sound probes and meteor studies. Rockets can now reach heights up to about 150 km and it is to direct measurements made from these that we may look for progress in this field. Unfortunately the difficulties are great. The velocity of the rocket is very high and direct thermometry of air temperature is impossible on account of the very high and largely unknown speed correction. The atmospheric pressure cannot be measured easily because, on account of the very large impact pressure, the static air pressure (as distinct from the total pressure which includes the impact

pressure) is very difficult to obtain. The pressure is also very low and vacuum manometers must be employed to measure it rather than the more conventional types of barometers with which meteorologists are acquainted. Finally, the recovery of instruments from the rocket is an awkward practical difficulty. A considerable effort is being expended on these lines in the U.S.A. and some results have been announced (Greenstein, Clearman and Durand 1949). No unexpected features have so far come to light.

Above 150 km the main sources of information are radio probes and aurorae and these mainly give information of the state of ionisation and excitation of the molecules and atoms, a subject usually regarded as outside the scope of meteorology. Aurorae are occasionally seen at heights as great as 1,100 km, though only when the atmosphere at the region of their formation is sunlit. There must, however, be a significant air density to produce the aurorae and the atmosphere can only extend to this height if the temperatures above 200 km are high, of the order of 1000-1500°K.

(e) *Measuring the constituents of the high atmosphere*

The problem of the chemical constitution of the high atmosphere was formerly investigated by theoretical methods alone, and several papers have been published on the gravitational settling out of the various constituents, but these calculations are not receiving experimental support.

Paneth and Gluckauf (1946) have developed a technique whereby small samples of air can be analysed and the fractions of the inert gases measured. Sufficient air is obtained by opening a small evacuated vessel and then resealing after a sample has been obtained. They have analysed air collected from free balloons released over South England. Samples collected from heights up to 20 km showed no significant increase in the helium content, but samples collected from between 20 and 25 km showed increases which averaged 3·3 per cent. The accuracy of the measurements was believed to be about 0·5 per cent. This increase of helium content of 3 or 4 per cent. would occur in truly stagnant air in a column of air about 300 m high, so evidently considerable mixing is occurring to prevent the separation. Even this modest increase however may not be real. Chackett, Paneth and Wilson (1949) have reported the analysis of a sample obtained by rocket from a height of 70 km. The sample was peculiar in that it contained no oxygen, but this may have been absorbed in the sampling vessel. The helium : argon : neon : nitrogen ratios, however, were the same as for surface air, so there may be no gravitational separation even as high as 70 km. It was stated that the sample may have been contaminated with surface air and too much weight, therefore, should not be placed on the one result.

E. Regener (1936) has also reported the analysis of a sample collected from 28 km. No change was found in the oxygen : nitrogen ratio at this level.

A considerable amount of data is now available concerning the spectra of the light from the night sky, and from aurorae. These show what excited elements are present, but the interpretation of the data in terms of the fractional composition of the atmosphere is not usually possible at present. If it were, great advances could be made, for there are several features to explain. Amongst these is the

ppearance of the sodium D lines in the light of the night sky. Why is there tomic sodium at 70 km ?

(f) *Cloud physics*

The processes which occur in fog and clouds are not only of interest as physical henomena, but they play a fundamental part in the formation of rain and thus h the economic life of the world. They are, therefore, of great importance, even hough the formation of cloud and rain can often be regarded as a secondary process n the more fundamental movements of the atmosphere.

Measurements which have been made on cloud elements fall into two main lasses : firstly, observations on the actual droplets or snow particles found in he atmosphere in fog or cloud, and secondly, laboratory experiments to determine he nature of the nuclei upon which droplets or ice crystals grow.

(g) *Cloud-drop size measurements*

As regards observations made in cloud, much ingenuity has been shown in evising ways of catching cloud droplets or ice particles to determine their size istributions, and in the case of ice, their shapes. Small cloud droplets can usually e collected on a suitably prepared surface by forcing the air against it. The air urns aside near the surface, but the drops impinge and are caught. Various surfaces an be used such as a vaseline/oil mixture of the right viscosity. The droplets re embedded in the oil and can later be examined under the microscope. Alter- atively a surface coated with magnesium oxide, or even smoked with ordinary oot, will register the contact of a droplet by a mark, the diameter of which bears a efinite relation to the size of the droplet. In the *replica* process, which is specially uitable for snowflakes, the slide is covered with a solution of suitable plastic dissolved n a solvent of moderate vapour pressure. The impression left after impact is hen preserved in the hardening surface film.

The sampling arrangements can be very simple ; in a fresh wind it is sufficient o hold the slide face to wind for a few seconds. At high speeds, as from aircraft, he catch becomes too large unless a shutter is employed to restrict the exposure o say 1/100 sec. On the other hand the counting, in which the drops or their mpressions are measured and counted under a microscope, is a most laborious rocedure. Devices to simplify this procedure include the cascade impactor May 1945) and the conifuge (Sawyer and Walton 1950). In both these instruments he drops are automatically separated out into size groups so that only counting s necessary.

In supercooled clouds ice formation occurs as rime on objects exposed to he wind or to the slipstream of an aircraft and this process can be used for drop-size neasurements, for small drops are swept round and are not deposited while arge drops have sufficient inertia to impinge. Cylindrical rods are used to collect he ice and are rotated so that the ice builds up uniformly and they retain their ircular form. The total ice deposited is then found by measuring the increase f diameter of the rods. The critical size of drop for collection, which depends pon the speed of the airflow and the diameter of the cylinder, can be calculated

by data given by Glauert (1940). Small cylinders are more efficient collectors *i.e.* they collect smaller drops than large cylinders, and by using several cylinders of different diameters, a rough size-distribution curve can be obtained for the drops.

By the application of these techniques, a fair picture of the sizes of drops in a cloud has been obtained and the distribution in different types of cloud is generally known.

These methods, however, all fail when the drops are large enough to break on impact and this failure is an impediment in rain-formation and aircraft-icing studies. There have been several attempts to avoid this by photographing the cloud drops from an aircraft. Even with modern high intensity, short duration, light sources such as the *arditron*, this is very difficult and until recently no one had succeeded. It is understood, however, that a successful camera has recently been made by H. W. Elliott of the *National Research Council* low-temperature laboratories in Canada. The arrangement depends on the shadowgraph principle whereby the drops are situated between the light source and the camera lens. To adapt this method to the photography of cloud droplets in flight, a high intensity short-duration spark-gap discharge is used as a source of illumination, combined with rotating prisms between the objective lens and the film in order to make the image of the droplet appear stationary rather than elongated in the direction of flight. The stationary image on the film is obtained by adjusting the angular velocity of the prisms to the true speed of the droplet relative to the camera. Completion of the electrical circuit and breakdown of the spark gap are arranged to occur only at coincidence of the prisms. A x7 magnification is provided by the optical system, since otherwise the image of a small droplet approaches the grain size of the film.

The use of a camera to obtain drop-size and size-distribution data has obvious advantages. The droplets are undisturbed in their motion, the operation of the camera is convenient and a large number of individual samples can be obtained in a short period and recorded permanently. A serious disadvantage is the small focal volume and the necessity for a large number of photographs for statistical analysis, which must inevitably be extremely slow and tedious. Moreover, there will be apparent variations in the size of the drops owing to variations in the sharpness of focus, and it is not clear whether these can be allowed for.

Nevertheless, with further improvements the cloud-drop camera may become a standard method and its successful development is very welcome. What would be more welcome is a method combining the advantages of the cloud-drop camera with some means of automatically sorting and counting the various sizes of drops.

A very useful new technique for " watching " the processes which occur in clouds is provided by radar. On the very short wavelengths of 3 and 10 cm the radio energy scattered by raindrops or snowflakes is large enough to be detected by radar techniques and the regions where such particles are plentiful give high scattering and can be shown as bright areas on a cathode ray screen. The behaviour and levels of these echoes is providing interesting data, but interpretation is difficult. For a discussion of radar echoes see Jones (1950).

(h) *Condensation and freezing nuclei*

Experiments on the nature and properties of the nuclei upon which cloud oplets and ice crystals form have shown considerable progress and continue in tive development. Aitken in his classic experiments first showed that spontaneous ndensation did not occur but that nuclei were necessary, and that these were esent in the atmosphere to a very variable extent. Much information on the ucleus content of the air has been obtained with the well known Aitken counter, owing that the numbers can exceed 10^4 cm^{-3} in cities and be as low as 1 or 2 cm^{-3} er the sea and in the upper air.

In the consideration of the nature and number of *ice* nuclei Findeisen, in a illiant series of experiments, lead the way, and it is now known that the number nuclei which can give rise to an ice particle is limited, though the number which n do so increases as the temperature falls. Considerable effort has been expended elucidate the details.

The basis of the experiments is the fortunate feature that if a cloud is brilliantly uminated the ice crystals have a very characteristic appearance and there is no fficulty in identifying and counting them. The water cloud has a dull, lifeless ppearance, but the ice crystals glitter in a striking and most attractive way, which akes each one obvious even to the unskilled observer.

For his experiments Findeisen used a large chamber of volume 2 m^3 ; this as cooled by a surrounding liquid bath and the air in the chamber was cooled fficiently to form a cloud by adiabatic expansion under a pump. Such ice crystals s formed were seen in the chamber and were counted. Findeisen (1943) has ublished curves of the variation with temperature of the number of nuclei per nit volume which give ice crystals at temperatures between —15°C and —35°C.

Fournier D'Albe made observations with a small expansion chamber, using h ultramicroscope to view the cloud particles. He could thus watch the way h which individual nuclei behaved but because of the small volume he could only ork with high nucleus concentration. He found that at temperatures below -41·1°C (the temperature is quite critical) most atmospheric nuclei yielded ice ystals and not water drops. He also found that when condensation occurred elow —41·1°C, though crystals were obtained, the growth only began when the ir became supersaturated with respect to water. It is therefore presumed that ondensation *must* start as water deposition but a drop thus formed immediately eezes and then acts as an ice nucleus. This type of nucleus is, therefore, generally eferred to as a freezing nucleus. True sublimation nuclei, which operate and row as ice immediately the air becomes supersaturated with respect to ice, have ot been found in the atmosphere.

The experimental evidence can therefore be summarised by saying that we now that the number of nuclei which will give rise to ice crystals increases as he temperature falls and we can plot some parts of the freezing nuclei/temperature urve for ordinary city air. Fig. 2 shows the results available. It is most unfor- unate that such observations as have been made have not been combined with rdinary Aitken nucleus counts to show the importance of the variation of the otal number of nuclei of all kinds. This would show, if one drew in the complete

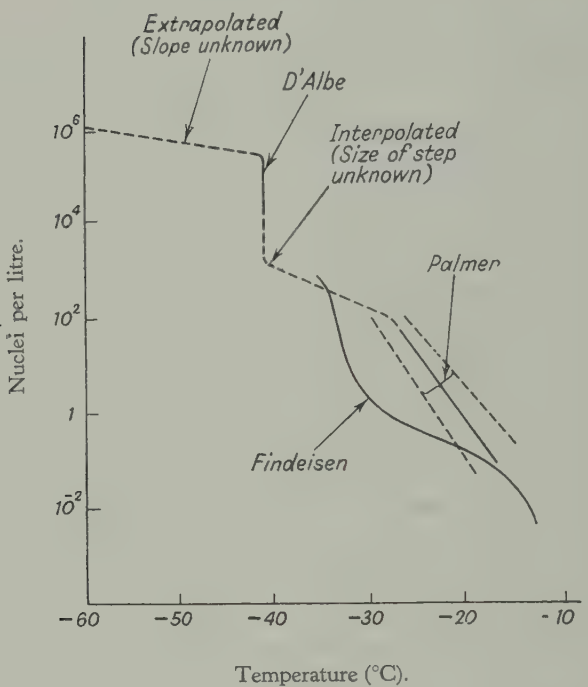

Figure 2. Observations of freeezing nucleus content in city air.

curve for different occasions, whether its shape changed materially from time to time or whether the curve merely moved up and down according to the total nucleus count. At present all the results available are consistent with this latter view, but since the total number of nuclei can vary through a range of at least 10^4, it is easy to imagine the total number adjusted to a suitable value to make any particular result reasonable. This particularly applies to Cwilong's observations at sea and on the Jungfraujoch (Cwilong 1948) and Palmer's observations in the upper air (Palmer 1948).

The number of freezing nuclei found even in the severely polluted atmosphere of a city, and operative at various temperatures, is quite inadequate to explain why a cumulonimbus cloud can become an ice cloud when the lowest temperature in it is near —15°C. This may be due to splintering of the ice crystals during growth, whereby many ice crystals can arise from only a few nuclei initially present. The process was first observed by Findeisen (1944). Unfortunately no way has so far been seen to establish experimentally whether it is, or could be, important in the atmosphere. The difficulties lie in the very variable nature of the effect and also in the great difference between the volumes which can be employed in any laboratory experiment and the scale af the atmospheric phenomena. The scale effect is a common difficulty in meteorological experiments.

(i) *Atmospheric turbulence measurements*

Atmospheric turbulence is a prime process in the atmosphere, for it is by s means that water vapour must be transferred from the surface of the earth the sea into the main body of the atmosphere, to be later condensed and returned rain by the major dynamic movements.

The observations made in the study of atmospheric turbulence include, on e one hand, the measurement of profiles of mean values of wind velocity, tem- rature and vapour pressure and, on the other hand, the measurement of the ctuations of these quantities in time and space. Numerous special instruments ve been designed for the profile measurements, such as the very light and sensitive emometer described by Sheppard (1940) and the thermocouple psychrometer paratus of Pasquill (1949). Using both these instruments Pasquill has obtained excellent series of measurements of the factors governing the turbulent exchange water vapour and heat in the lowest layers of the atmosphere.

For the other approach to the investigation of turbulent transfer near the ound, Swinbank (1950) has devised apparatus for measuring the simultaneous ctuations of temperature and of the vertical component of eddy velocity, using t-wire anemometer and thermocouple techniques. At the Imperial College of ience (Sheppard and Charnock 1950) a distinct advance is now being made by e design of instruments to measure the product of simultaneous fluctuations of mperature and other meteorological factors (it is the product which is ultimately quired in these investigations), using an integrating mechanism which will give e arithmetical value for the flux of the various entities.

Another type of measurement of importance in work on atmospheric turbulence that of the drag of the wind on the earth's surface, the first reliable measurements which were made by Sheppard (1947). Previously no direct measurements of s quantity had been made, though L. F. Richardson had made shrewd estimates observing the deflection of ears of corn when swayed by the wind! Sheppard easured the drag on a flat plate floating in a liquid contained in very shallow bath, e whole being faired into the surface of the surrounding smooth ground as carefully possible. The drag plate was held in the centre of the liquid by a rigid torsion m which was attached to a suitable vertical torsion head. The drag on the plate nded to move it and so twist the phosphor-bronze torsion wire. The twist taken the wire was measured by a telescope and scale method and it was calibrated dependently in terms of horizontal force on the plate. Measurements could only made in light and moderate winds ; in strong winds conditions were not suffi- ntly steady. For surface wind speeds of about 4 m/sec the drag or shearing ress was found to be 0·2 to 0·3 dynes/cm^2. This is of course the drag on the nooth surface (the work being designed to establish the *law* of drag) and may be mpared with a value about four times as large obtained by Pasquill (1950), using a nilar method, for a grassland surface. If the drag were everywhere as low as at over the smooth surface, storms would be a very long time dissipating their netic energy.

Although it is now usual to regard the horizontal shearing stress as independent height near the ground, no direct measurements to confirm or contradict this ve been made since those of Scrase (1930) who evaluated the shearing stress from

cinematograph records of the movements of very light vanes and found a marked variation with height, though observations were not simultaneous. As most theories of turbulence depend on the fundamental assumption of shearing stress being independent of height, it is rather surprising that other experimental evidence has not been forthcoming.

4. Conclusion

In this review it has not been practicable to cover anything like the whole field of meteorological measurements and our attention has in the main been focused on those methods and developments which have not, so far, found their way into the textbooks. However, even with this limitation, it would require much more space to cover all the newer ground adequately, and it is to be hoped that some of the gaps which we have left will be filled in by the succeeding articles in this series, since they are certain to involve discussion of measurements of one sort or another.

In meteorology, more than in any other science perhaps, there is a tendency for measurement and experimental work generally to necessitate the use of teams of workers and a good deal of organisation. The outstanding example of this trend is the *Thunderstorm Project* which was undertaken in America in 1946-7 by four U.S. Government Agencies. This project, the final report of which has recently been published under the names of Professor Byers and Mr. Braham (1949) involved the setting up of a network of 55 special stations about a mile apart for surface observations, 6 radio-sonde and wind stations, the use of several specially fitted aircraft, radar equipment and a variety of special instruments. The results achieved appear to have well justified this intensive effort. Nevertheless, it is to be hoped that the day is not altogether past when the individual experimenter, such as an Aitken or a W. H. Dines, can pursue his investigations and produce from relatively simple equipment important results for meteorology.

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Observations of rain from non-freezing clouds

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Summary

A description is given of observations made in south-east Australia of medium and heavy rain falling from clouds which were everywhere warmer than 0°C. Detailed observations were made visually from aircraft and these were supplemented by measurements of the radar echo intensities and drop size spectra within the clouds.

The clouds were all deep cumulus and were over or near the sea. Great concentrations of relatively large water drops were observed in the upper levels of the clouds.

1 Introduction

There are differences of opinion amongst meteorologists as to the intensity of precipitation which falls from non-freezing clouds. Findeisen (1939) is of the opinion that only light drizzle can fall from such clouds, and Bergeron (1933) states that " almost every real raindrop and all snowflakes originated around an ice crystal". On the other hand, in a recent paper, Leopold and Halstead (1948) state categorically that " the majority of tropical convective showers come from clouds which do not reach into sub-freezing temperatures". Some textbooks of meteorology, e.g. Petterssen (1940), recognize the possibility of rain from non-freezing clouds but seem doubtful as to the cause. One reason which may account for lack of belief in and understanding of appreciable rain from non-freezing clouds is the paucity of published data on the phenomenon. Simpson (1941) was able to quote only two examples. Three other brief descriptions have since been given by Vexler (1945), Kotsch (1947) and Hunt (1949).

A number of observations of rain from non-freezing clouds have been made in Australia by the author and others, and some of them are described in this paper in as much detail as possible. They were made in the course of flights, the purpose of which was the artificial production of rain ; the frequency of observations bears no relation to the frequency of occurrence of the phenomenon.

2. General description of flights

These flights took place in the neighbourhood of Sydney. The aircraft was supplied and flown by a special detachment of the Aircraft Research and Development Unit of the Royal Australian Air Force, and carried C.S.I.R.O. observers. It operated from Richmond aerodrome, some 30 mi NW. of Sydney. Soundings accurate to 1°C were taken in the aircraft, and radiosonde data were available from ascents at Rathmines, near the coast 70 mi N. of Sydney.

Three flights are described in which rain was observed to fall from non-freezing clouds. In the first and third cases, observations were made visually and by radar, of heavy and moderate rain falling from cumulus ridges. One was over the sea

and one over the land near Sydney. In the second case, drop-size distribution measurements were made at various heights in a cloud from which light rain was falling.

3. Flight on 7 April 1948

(a) *General meteorological situation*

An elongated anticyclone lay with its major axis from the east coast of Tasmania to the north coast of New South Wales. The winds at Sydney and Rathmines below 10,000 ft were light and variable, mostly from SE. to NE. and less than 10 kt. A number of secondary cold fronts, orientated NW.-SE., had moved up the western Tasman Sea on the preceding days. One of them could have become quasi-stationary in the area of the observations where the state of the atmosphere is probably represented fairly well by the radiosonde at Rathmines at 2100 hr on 7 April (Table I). The aircraft temperature observations were in good agreement with the radiosonde, and confirmed that at the inversion at 7500 ft, the lowest temperature reached was + 3°C.

TABLE I.—Sounding 7 April 1948 Rathmines radiosonde 2100 hr

Height (1,000's ft)	..	1	3	4	5	7·5	9	10	11
Temperature (°C)	..	18	13	11	11	3	7	6	5
Dew point (°C)	..	15	10	9	7	3	—6	—18	—20

(b) *Observation of clouds*

A flight was made in the afternoon to the area of the observations, about 110 mi NE. of Sydney and about 30 mi out to sea. There was an almost unbroken layer of stratocumulus cloud, which extended more than 50 mi in all directions. Its top was at about 7,500 ft, limited by the inversion, and its base at about 4,000 ft. Through this protruded a ridge of cumulus, which was the scene of most of the observations. It was about 3 mi wide and more than 30 mi long, aligned WNW.-ESE. It sloped gently upwards to the ESE. where the top was at 10,000 ft, then abruptly ended. Observations were concentrated on the ESE. or higher end, referred to hereafter as *Area A*.

Another ridge of cumulus protruded through the stratocumulus layer. It formed, roughly, a continuation of the first ridge and was about 60 mi away. This ridge was not investigated, but it was estimated to have the same maximum height as the first, namely 10,000 ft. About 30 mi to the SW. of the first cumulus ridge there was a patch where the top of the stratocumulus layer was noticeably ruffled. Rain fell in this area, referred to as *Area B*.

The aircraft flew to Area A, the ESE. end of the nearer ridge of cumulus, at a height of 10,000 ft, arriving at 1547 hr. There was a small space of clear air between the stratocumulus layer and the southern side of the ridge, but on the other side the stratocumulus continued unbroken and merged with the ridge. A cross section illustrating these features is given in Fig. 1. The top of this ridge had a

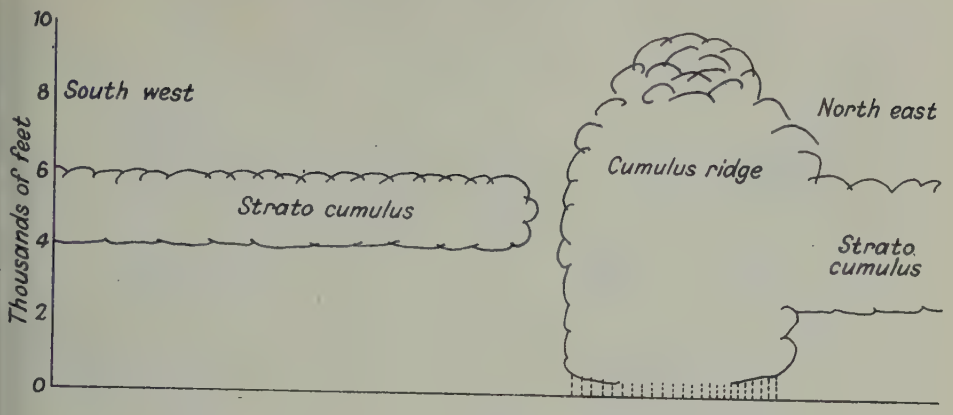

Figure 1. Cross section through the clouds, 7 April 1948.

number of characteristic features ; for instance at the eastern end there were two peaks at heights of 10,000 ft and 9,500 ft, separated by a level area at a height of),200 ft. The temperature in the clear air outside the cloud was +6°C at ,500-10,000 ft.

(c) *Observations of rain*

The aircraft descended to the bottom of the cloud through the gap at the outhern side, where the side of the cloud was almost vertical from the top right down to the sea. A coloured rainbow was seen near the top of the cloud on the way down, which suggests (Geddes 1939) that here there were quite large water drops of 1 mm diameter or more. The aircraft arrived at the bottom at 1606 hr nd heavy rain was seen to be falling from the whole ridge. It extended right out o the side of the ridge and all along its length, from the extreme eastern end as ar as could be seen. Some 15 mi from the end there was a gap in the rain about mi wide. The heaviest rain, which was opaque, was seen under the highest loud at the ESE. end of the ridge. The base of the cloud ridge here was at about 00-1,000 ft at the edges and appeared to be lower in the middle. The base of he stratocumulus layer was at about 4,000 ft, and there was no general precipi- tation from it.

Another patch of rain could be seen about 30 mi away to the SW. ; it appeared o be of moderate intensity. Its position coincided with the ruffled tops of the tratocumulus layer, referred to above as Area B. The size of this patch was f the order of 5 mi each way. No other rain could be seen, although the visibility as at least 50 mi and the coast could be plainly seen. There was no general precipitation from the stratocumulus layer.

At 1609 hr the aircraft flew through the edge of the rain at the extreme eastern nd of the ridge, Area A, where the rain was less intense than in the middle. It as moderate to heavy, making a loud noise as it hit the aircraft and flowing off he windscreen in a continuous stream. The pilot was not prepared to fly through he densest rain, considering it to be so heavy as to constitute a hazard to flying.

(d) *Flight through the cloud*

After circling the rain at the bottom of the ridge, at Area A, and flying through the gap mentioned above, the aircraft returned to the top, climbing through the clear space on the southern side of the cumulus ridge. The appearance of the top was unchanged. The characteristic features mentioned above were still there and were at the same heights. At 1635 hr the aircraft flew through the cloud on a southerly course, about 200 ft below the top. A large number of water drops of appreciable size hit the windscreen, resembling in size light rain rather than normal cloud particles*. Qualitatively it would be classified as " very wet for a cumulus cloud." No trace of ice was observed. The air was remarkably free from turbulence. Inside the cloud the bumps which the aircraft encountered were fewer and of much less intensity than is usual in cumulus clouds, while outside the cloud the air was completely smooth.

The aircraft remained at the top of the cloud until 1638 hr, by which time there was still no change in the appearance or height. It then returned to the bottom, arriving at 1650 hr. The rain was observed until 1655 hr, after which the aircraft returned to Richmond. The rain by then was falling from the same areas as before, but the intensity was less. That under Area A, the end of the ridge, was estimated as moderate to heavy, while further along the ridge (past the gap 15 mi from the end) and at Area B, the rain was light to moderate.

(e) *Radar observations*

The flight was observed by a 10-cm radar set at George's Heights, near the entrance to Sydney Harbour. Observations were commenced at 1545 hr, and continued until 1615 hr. The aircraft, which was in Area A, was at too great a range to be seen. Immediately the set was switched on, rain echoes were observed in two areas which correspond accurately with Area A (obtained by dead reckoning on the aircraft's flight) and Area B, as estimated relative to Area A. Both echoes appeared almost to saturate the P.P.I. At 1555 hr the echo at Area A increased in strength and grew in size, reaching a maximum at 1602 hr, after which it slowly diminished. By 1615 hr both echoes were quite faint. At the range corresponding to Area A, the lowest height of a target from which the radar can receive an echo is about 4,000 to 5,000 ft, so the echoes must have been reflected from the upper part of the cloud ridge. The lowest height at which a target is visible in Area B is about 2,000 ft. Calculations, based on the refractive index of an atmosphere represented by the sounding shown in Table I, show that these figures could not have been affected by reflection at the inversion.

The way in which radar echo intensity depends on the number and size of raindrops has been described by Ryde (1946). From a given echo, neither of these quantities can be determined unless the other is measured or assumed. But for an assumed drop size the drop concentration or water content can be computed

* When an aircraft flies through clouds or rain, water may be deposited on it and can be seen as it hits the windscreen. The chance of the aircraft hitting a drop in its path depends on the size of the drop (Langmuir and Blodgett 1944). In the case of an aircraft, most drops larger than about 0·1 mm diameter hit the aircraft while most smaller drops are carried round it by the air stream. Thus the rate at which water hits the windscreen of the aircraft is an indication of the water content of the cloud in the form of large drops.

om Ryde's formulae. This has been done for the radar echo intensity on this casion, for a range of drop sizes, assumed to be uniform. The precipitation rameters could have been as follows :—

Assumed drop diameter in mm	1	2	5
Corresponding water content, gm m^{-3}	40	5	0·3

In practice, the drops would not be all of the same size. If the drop-size stribution was similar in shape to that measured by Laws and Parsons (1943) r rain, then for a given radar echo the water content would have been about one-th as much as that quoted above, drop diameter here referring to that at the ak of the distribution curve, the size in which most water is concentrated. It ems clear that, whatever the drop-size distribution, the conclusion can be drawn at either there were, in the top half of this cloud (Area A) many large water drops the order of 2 mm in diameter, or that if the drops were smaller the water content ust have been extraordinarily large.

Owing to a misunderstanding, 60 lb of granulated dry ice were dropped into e cumulus ridge at Area A at 1547 hr when the aircraft first arrived. This could t have caused the rain, since it was observed on the radar before the dry ice was opped, and the rain, when first observed at 1606 hr, covered so large an area. ny effects of the dry ice could hardly have spread so far in so short a time. Further, l parts of the cloud were above 0°C. The radar echoes, whose intensity increased ortly after the dry ice was dropped, are not inconsistent with the dry ice (or the esence of the aircraft) having caused a temporary increase of rain intensity.

4. Flight on 1 June 1948

(a) *General meteorological situation*

An anticyclone lay over eastern South Australia and western New South Wales, d a slow south-south-west air stream was moving up the western Tasman Sea. he aircraft sounding on the climb eastwards from Richmond showed a uniform pse rate approximately equal to the wet adiabatic.

(b) *Description of clouds*

A flight was made in the afternoon to a cumulus ridge over the sea. It was raight, and extended ENE. from a point 40 mi south of Sydney to well over 150 mi it to sea. Its orientation and shape suggested that it lay along a degenerating ld front. To the north and west of the ridge there was no cloud, while to the uth and east there was scattered cumulus. The top of the ridge sloped gradually wards from 4,000 ft at the western end to 8,400 ft at the scene of the observations, 0 mi out to sea. The base was at 2,500 ft at the western end but was lower it to sea ; at 80 mi out it was at 2,000 ft and at 100 mi, in the rain area, it was 1,000 ft.

The aircraft flew along the ridge level with the top and at 1500 hr arrived at a irly prominent peak in the ridge with its top at 8,400 ft, where the temperature the surrounding air was +1°C. Beyond this peak, further along the ridge,

the cloud top sloped down to about 7,000 ft, but 20 mi away there were other peaks as high as the one observed. A cross section through the ridge at this point is shown in Fig. 2. To the south-east of the ridge there was scattered cumulus ; to the north-west there was a layer of stratus at 6,000 ft extending 2 mi from the ridge, and at 2,000-3,000 ft a layer of fractocumulus, which extended 4 mi from the ridge.

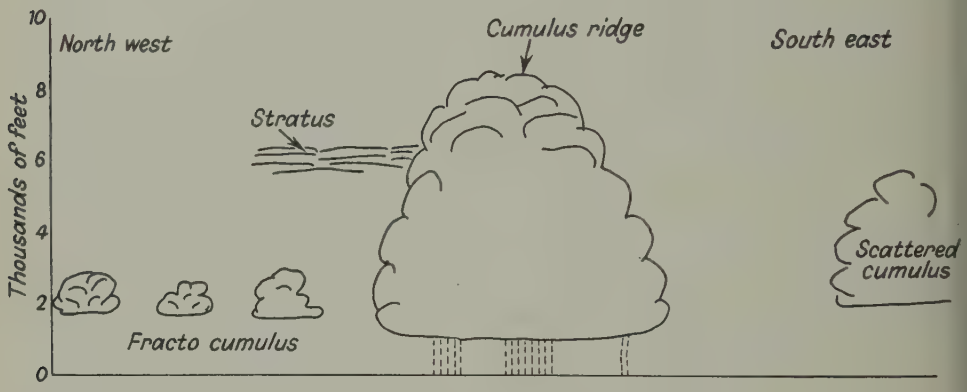

Figure 2. Cross section through the cloud ridge, 1 June 1948

The fact that the temperature of the clear air level with and near to the top of this cloud was measured as +1°C does not rule out the possibility that the cloud-top temperature was less than 0°C. Nevertheless a description is included because many of the observations make it appear probable that rain which fell from this cloud was formed by the same process as that observed on the other two flights, and because flights through the cloud at various levels yielded additional information on water-drop distribution.

(c) *Rain areas*

The aircraft flew to the bottom keeping in the clear air to the north of the clouds. Patches of rain could be seen falling from the bottom of the ridge, the heaviest patches being under the highest peak. The aircraft flew through several patches of this rain ; they varied in intensity and the heaviest were estimated as light to moderate.

(d) *Flights through the cloud*

The aircraft then flew through the cloud at 6,000 ft, 7,000 ft and 8,000 ft. The top of the cloud was observed at 1535 hr ; it was still at 8,400 ft and there was no change in its general appearance. Inside the cloud there was no appreciable turbulence, while the surrounding air was calm. The cloud was much wetter at the top than lower down. At the top, *i.e.* 8,000 ft, there was a definite impression as of the aircraft being struck by rain of about twice the intensity of the heaviest rain beneath the cloud. The drops were almost as large. At 7,000 and 6,000 ft,

wever, there was much less water in the form of drops large enough to hit the craft. No trace of ice was observed either to hit the aircraft or to form on it. e flights through the cloud were made as far as possible in the same vertical ne, that of the highest peak and heaviest rain, and were all made at right angles the ridge.

During these flights through the cloud, approximate measurements were made drop size, drop density and rain-water content. In the instrument used for s purpose the raindrops impinged on sensitive paper and were recorded as spots. strip of the paper 4 in. wide was moved at a steady speed of 2 ft sec^{-1} past a slot 5 in. wide exposed to the air stream about one foot outside the aircraft's fuselage. e relation between spot size and drop size was found by calibration, the minimum op diameter capable of producing a record being about 0·1 mm. Thus, drops corded by this instrument are too large to be unduly deflected by the divergent w of air round the aircraft. This instrument was, at the time, in an early stage development, and measurements of drop diameter obtained by its use must be garded as approximate. It is, however, thought to give reliable comparative ures of the drop-size distribution and water content at the various levels within cloud. The measurements are displayed in Table II. A large concentration drops of about 0·4 mm diameter is shown at 8,000 ft. In the rain under the ud, the drops are somewhat larger but the drop density and water content are aller than at 8,000 ft.

II.—Observations of drop concentration and water content in cumulus cloud on 1 June 1948.

Estimated diameter (mm)	<0·2	0·2 to 0·4	0·4 to 0·6	0·6 to 0·8	0·8 to 1	1 to 1·2	1·2 to 1·4	1·4 to 1·6	Totals
Drop density m^{-3}	54,000	9,200	1,300	220	18	—	—	—	64,840
Water content mg m^{-3}	25	130	85	40	7	—	—	—	287
Drop density m^{-3}	14,000	1,100	130	18	—	—	—	—	15,250
Water content mg m^{-3}	7	15	8	3	—	—	—	—	33
Drop density m^{-3}	9,000	400	50	10	—	—	—	—	9.460
Water content mg m^{-3}	5	6	3	2	—	—	—	—	16
Drop density m^{-3}	15,200	1,160	480	220	70	14	5	2	17,150
Water content mg m^{-3}	7	16	31	40	27	10	6	4	141

5. Flight on 24 March 1950

(a) *General meteorological situation*

A depression was centred about 500 mi south of Sydney and a trough of low essure extended along the New South Wales coast, with scattered rain. Surface nds were southerly and there was probably a north-westerly air-stream above 000 ft. A sounding is given in Table III in which temperatures were measured the aircraft near the cloud on which observations were concentrated, while dew ints were taken from the radiosonde at Rathmines, 8 hr later.

TABLE III.—Sounding 24 march 1950

Height (1,000's ft) ..	1	2	3	4	5	6	7	8	9	10	11	12	13
Temperature (°C) ..	18	16	14	14	12	10	8	7	8	7	6	5	4
Dew point (°C) ..	16	14	12	11	9	7	4	−2	−13	—	—	—	—

(b) *Description of clouds*

Over the coastal region near Sydney there was broken cumuliform cloud from 1,500 to 8,500 ft. Layers existed from 1,500 to 3,000 ft and from 7,000 to 8,500 ft, both covering about $\frac{5}{8}$. In addition there were scattered patches of cumulus at all intermediate heights. Over the sea the cloud was more scattered but of a similar general nature. Along the coast there were a few more substantial cumuli extending in a solid mass up to and sometimes through the inversion at 8,500 ft. Observations were concentrated on the most prominent of these, which was in the form of a ridge, 18 mi long by 5 mi broad, aligned WNW.-ESE. Its base was at 1,500 ft and top at 9,000 ft (+8°C) at the eastern end. At the western end it was unbroken up to 10,000 ft (+7°C) above which there were several humps, about a mile wide and 1,000 to 1,500 ft high. They were closely packed, merging together at their bases. They rose and fell, the height of the tops varying between 10,500 ft (+6·5°C) and 11,500 ft (+5·5°C). The period of rise and fall was of the order of 10 to 15 min. The cloud top was observed from 0955 hr to 1040 hr, and presented the same general appearance throughout this time, but the detailed structure of the top was continually changing. The western end of the cloud ridge was surrounded by almost unbroken altocumulus, but there were spaces through which the ground could be seen around the eastern end, which was over Port Hacking, 15 mi south of Sydney.

With the solitary exception of a large cumulonimbus at least 100 mi to the NE., no higher clouds were observed.

(c) *Radar observations*

The aircraft flew backwards and forwards over the top of the cloud from 1008 hr to 1040 hr at 12,000 ft except for a descent to 11,000 ft at 1019 hr to check the height of the top of the cloud. During this period, observations were made of rain within the cloud by means of a radar set fitted to the aircraft.

The radar equipment consists basically of an SCR717, operating on a wavelength of 9·1 cm. An antenna is mounted in the nose of the aircraft, rotating at 20 revolutions per minute about an axis parallel to the line of flight. Echoes are displayed on a cathode-ray tube, similar to a normal P.P.I., but representing a cross section of space in a vertical plane transverse to the flight path. Photographic records are made of the display at each revolution ; a sample picture with its interpretation is given as Fig. 3 (a) and (b). The bright spot in the centre represents the aircraft : below it there is a bright area representing rain. At the bottom there are echoes from the ground. All these appear in the correct relative positions, the vertical and horizontal scales being the same. A series of successive display

ptographs, taken as the aircraft flies over or past the rain, enable three-dimen-nal measurements to be made of its extent. A second cathode-ray tube enables ensity measurements to be made in a direction which is indicated by the bright tor on the photographs (Fig. 3).

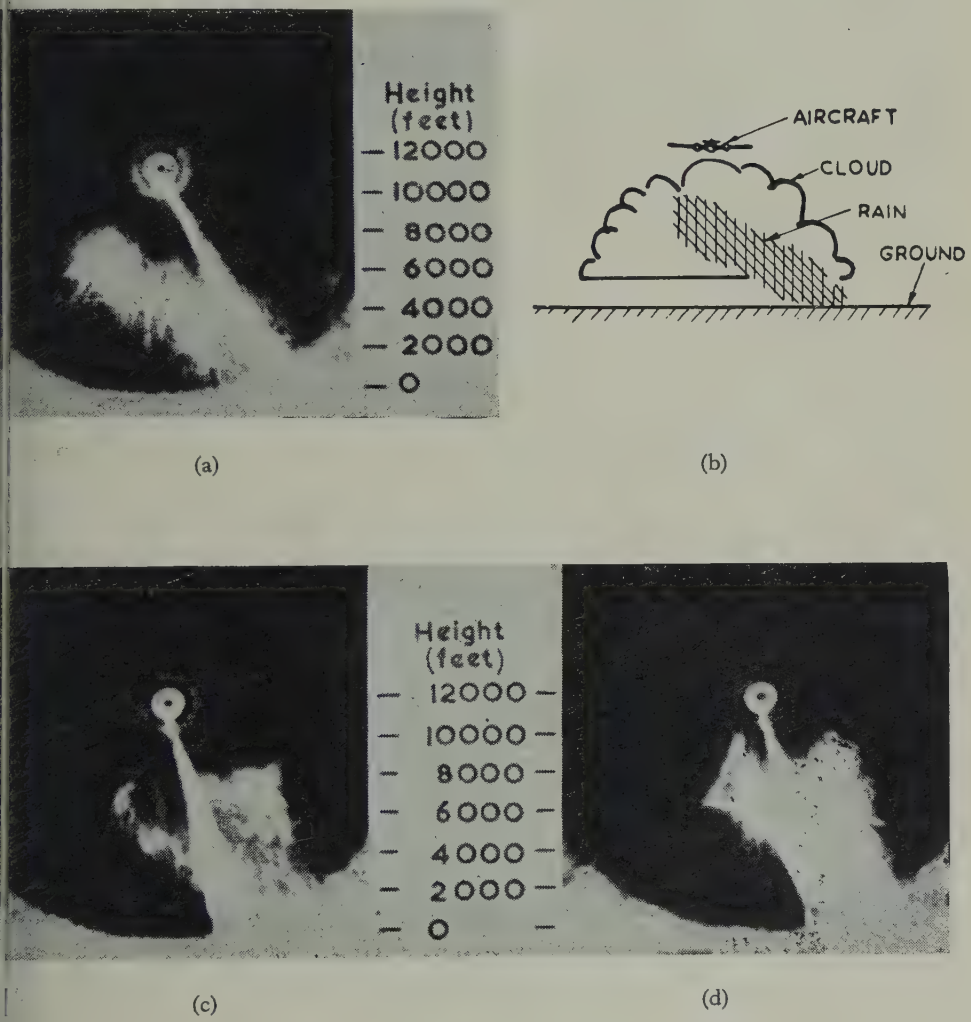

Figure 3. Radar display, 24 March 1950.

	Time	*Height*	*Course*
(a)	1020 hr 00 sec	11,400 ft	180°
(b)	Interpretation of (a)		
(c)	1011 hr 15 sec	11,900 ft	240°
(d)	1011 hr 00 sec	11,900 ft	240°

Five flights were made over the top of the cloud ridge and on each occasion rain echoes were observed, originating in the western end. They showed that the rain extended from the ground to 8,000 or 10,000 ft and took the form of columns distributed through the cloud. These were about 0·5 - 1 mi wide and were leaning at an angle of 45° to the vertical, the tops being to the SE. of the bottoms. This effect is clearly shown in Fig. 3 (a). The tops of the columns were in or under the humps on top of the cloud. On many occasions, *e.g.* in Fig. 3 (c), the echo showed a noticeable discontinuity at 8,500 ft, the height of the inversion, while often (Fig. 3 (d)) small columns of rain protruded above it. The echoes sometimes indicated that the rain extended almost up to the top of the cloud, the highest recorded being 11,000 ft. Echo intensity measurements, interpreted as described by Ryde (1946), show that if the raindrop size distribution was as described by Laws and Parsons (1943) the precipitation rate at the ground was about 4 mm hr^{-1}. If, as suggested below, the drop size was unusually small, the radar echo intensity corresponded to a higher precipitation rate.

(d) *Flight through the top of the cloud*

At 1038 hr the aircraft flew through the top of the cloud, on an easterly course, passing through two of the humps, whose tops were at 10,500 ft and 6·5°C. The aircraft slowly descended and passed 200 ft below the top of the first and 500 ft below the top of the second cloud ; in both, a great deal of water struck the windscreen. Drop diameter was estimated as 0·25 mm. Turbulence was moderate, bumps of about 0·5g being experienced. The cloud top was opaque, the sun only being seen as the aircraft was entering and leaving. No indication of the presence of ice crystals was seen at any time.

(e) *Observations of rain*

The aircraft then flew to the bottom of the cloud, keeping in a clear space to the north-east, and arriving at the bottom at 1050 hr. Showers of rain were falling from the western end of the cloud ridge and reaching the ground. Several could be seen, each covering an area of about 0·25 mi^2 and separated by a mile. No rain was falling from the surrounding clouds, visibility being about 30 mi. At 1100 hr the aircraft flew through two of the patches of rain. The intensity was estimated as moderate and moderate to heavy respectively. Water content was high, the water flowing in continuous sheets off the windscreen, but the drop size appeared to be somewhat smaller than is usual for such heavy rain.

(f) *Ground observations*

The area in which the rain was observed to fall was visited the next day and the inhabitants were interrogated. Much of the country was uninhabited, but a strip of land down the western side of the rain area was thickly populated. Rain fell here between 1000 and 1200 hr, mostly at about 1030-1100 hr. It was confined to a band 5 mi from north to south, and fell in scattered, fairly heavy showers lasting about 10 min to one hr. Two rain gauges recorded ·12 in. and ·44 in. during this period.

6. Conclusions

The observations of 7 April 1948 and 24 March 1950 provide positive evidence heavy rain can fall from a cloud which is everywhere warmer than freezing. observations of 1 June 1948 provide some additional evidence as to the processes work.

On all occasions the rain fell from deep cumuliform clouds over or near to sea. Relatively large water drops in great concentration were observed near top of the clouds.

Bowen (1950) has described how rain can form by coalescence, without the phase being involved. His theory appears to be supported by the observations cribed in this paper ; in particular by the observations of large numbers of tively large drops in the upper levels of the clouds, and by the type of cloud n which the rain fell.

Acknowledgments

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Grateful acknowledgment is made to the Royal Australian Air Force for the of the aircraft, and to the R.A.A.F. pilots who flew them.

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A note on the eddy transport of angular momentum

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SUMMARY

A pilot study of the horizontal eddy transport of angular momentum in the meridional direction, based on radio wind observations over the North American sector of the northern hemisphere, is presented. The results are in harmony with general frictional requirements except that the transports are rather large. The desirability of much more extensive compilations of data concerning the subject is indicated.

1.

The problem posed by the angular momentum balance of the earth-atmosphere system is probably one of the most fundamental considerations in meteorological science. In view of the fact that within the last few decades an extensive body of proper observational data has been accumulated, it is becoming possible to do more than merely speculate abstractly in this regard. As a result a number of papers, both observational and theoretical, have appeared relating to this matter. A selection of these is contained in the annotated list of references at the end of this note, although many others should be included in a more complete bibliography.

The particular question to which much attention is directed currently relates to the manner in which the necessary meridional transfer of angular momentum takes place within the atmosphere. Is this flow accomplished primarily through the agency of the so-called mean meridional circulations, or primarily through horizontal exchange processes ? The nature of our approach to the subject of the general circulation depends largely upon the answer to this question.

In order to seek enlightenment, recourse must be made to data. One may try to measure either one or the other of the two processes mentioned. The writer and his colleagues working with the general circulation project at the Massachusetts Institute of Technology have been engaged in studies which aim at measuring the eddy processes. Thus Widger (1949) made a hemispheric analysis of winds computed from isobaric maps for the month of January 1946 and obtained substantial indications of the importance of the eddy processes in the lower and middle troposphere.

Owing to the fact that isobaric maps for relatively high levels in the atmosphere are of uncertain reliability and are still difficult to construct, it is a tempting matter to utilize direct radio wind measurements for the purpose at hand. However, since such wind data are not available on a hemispheric basis, great care must be exercised in treating limited amounts of observational material in order to obtain meaningful results. Specifically it is essential to sample a significant and representative sector of longitude. Also, it is necessary to use averages over appreciable intervals of time to improve reliability, because of the effects of missing reports

* This research was performed under contract with the Cambridge Research Laboratories, U.S. Air Force.

d other factors such as the higher *noise level* in actual wind data. The last item s been discussed by Charney (1947). Selectivity in favour of light winds is ubtless also a factor, especially in the higher reaches of the soundings. Finally, like wind data secured from isobaric maps, the radio winds are available only stations arranged in an irregular array over the weather map, so that some appro- iate scheme must be devised to approximate the momentum transports across ividual latitude circles.

2.

It is by no means altogether clear how the needed calculations should be nducted to best advantage. For this reason the writer desires to enlist the advice the readers of this *Journal*.* For purposes of orientation an admittedly rough d restricted pilot study is presented below to serve as a basis for discussion. is probable that a more comprehensive project will be undertaken at a later date hen the best mode of procedure is apparent.

A beginning in the proper direction has been made in an interesting paper Priestley (1949), although it appears that a more elaborate plan is required. is method consists of first computing the time-average value of the product of e eastward and northward wind components, $\overline{uv}$, for each individual station at ch selected pressure level. From each of these quantities the contribution due the net meridional transport of air at the station and level, namely $\bar{u}\,\bar{v}$, is sub- acted, the bars again denoting time averages. The difference $\overline{uv} - \bar{u}\,\bar{v}$ is thus lated to the temporal fluctuations of the wind and would be zero under steady nd conditions. Since standing eddies, *i.e.* semi-permanent or average features of e general circulation, contribute to the meridional eddy flux of physical properties, is necessary to include their effects also by some modification of the scheme.

We may, on the other hand, use averages both over time and over a suitable gment of a latitude circle at a given pressure level, and use the bar to denote ch a space-time average. Thus $\overline{uv}$ now gives us a measure of the total momentum nsfer across the segment. A portion of this total may be due to a net flow of r across the segment, $\bar{v}$. If an entire closed latitude circle were considered, $\bar{v}$ ould for practical purposes represent a meridional circulation the effects of which is desired to exclude, and therefore $\overline{uv} - \bar{u}\,\bar{v}$ would be the needed quantity. For a rge but limited segment $\bar{v}$ may also include the effects of standing eddies of larger mensions than the segment chosen. Under these circumstances the subtraction $\bar{u}\,\bar{v}$ from $\overline{uv}$ may still eliminate effects of very large standing eddies, but the rocess does at least take partial account of the average features. A more detailed xamination of this use of space-time averages has been given by White (1950).

3.

In the pilot study all the soundings for the hours 0300Z and 1500Z reported the *Daily Upper Air Bulletin*, U.S. Weather Bureau (1949) at 52 stations during ebruary 1949 were used. As indicated by Roman numerals on the map in Fig. 1, e stations were separated into six groups according to approximate 10° latitude

* The pages of the *Journal* are always open for correspondence on this topic.—EDITOR.

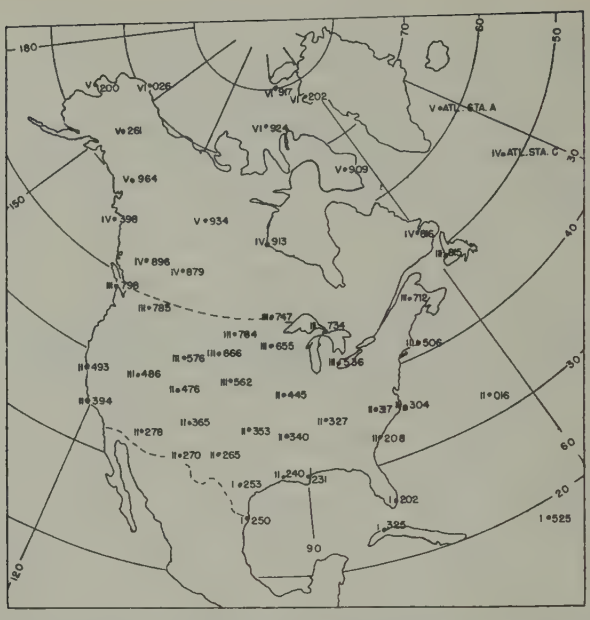

Figure 1. Map showing stations used in study. Roman numerals indicate classification according to approximate latitude belts. International station numbers entered to right of each station.

belts. The space-time average for a given latitude belt and given level was taken as the sum of all the observed values of a quantity at the several stations in the belt divided by the number n of such observations. This is rather crude, since the available observations are not uniformly distributed in space or in time.

The results are contained in Table I and Fig. 2, which are almost self-explanatory. The transports obtained may be looked upon as being given per unit mass in the vertical and per unit distance zonally, and hence may be integrated vertically with respect to pressure. These integrals were evaluated from graphs by means of a planimeter, the limits being from 100 to 1,000 mb. Furthermore, the total transports of linear momentum so obtained were converted into transports of angular momentum for a complete latitude circle in each case through the introduction of proper factors involving the earth's radius and the average latitude $\bar{\phi}$. Such integrated transports are given in cgs units in the last column of the table. The diagram in Fig. 2 gives the corresponding profiles showing the vertical distribution of these same transports of angular momentum. In effect the values in the last column of the table are proportional to the integrated areas of these profiles in the figure.

The writer's colleague Dr. W. K. Widger has made an estimate of the normal rate at which angular momentum is removed from the atmosphere through surface effects during February. For the entire polar cap north of 35° this rate appears to be about 10×10^{25} cgs units of angular momentum/sec. Upon comparison of this figure with the integrated transport for Group II, it is seen that the latter is about four times larger than the former. Various reasons may be present to

TABLE I. ANALYSIS OF RADIO WIND SOUNDINGS FOR FEBRUARY 1949

ess b	n	$\bar{u}$ m sec^{-1}	$\bar{v}$ m sec^{-1}	$\bar{u}\,\bar{v}$ m^2 sec^{-2}	$\overline{uv}$ m^2 sec^{-2}	$\overline{uv}-\bar{u}\,\bar{v}$ m^2 sec^{-2}	Integral gm cm^2 sec^{-2}
			GROUP I $\bar{\phi}$ = 25°N.				
00	30	+ 4·57	− 2·93	−13·39	+ 2·20	+15·59	
00	104	+21·21	− 1·76	−37·33	+18·09	+55·42	
00	177	+18·20	+ 2·28	+41·50	+91·33	+49·83	
00	252	+11·20	+ 4·31	+48·27	+91·65	+43·38	$+56{\cdot}0 \times 10^{25}$
00	273	+ 3·89	+ 3·55	+13·81	+26·68	+12·87	
50	275	− 0·37	+ 2·63	− 0·97	+ 8·06	+ 9·03	
00	234	− 3·21	+ 0·90	− 2·89	− 0·99	+ 1·90	
			GROUP II $\bar{\phi}$ = 35°N.				
00	36	+17·94	+ 2·58	+46·29	+46·69	+ 0·40	
00	148	+25·21	+ 1·42	+35·80	+85·39	+49·59	
00	314	+23·29	+ 0·66	+15·37	+68·82	+53·45	
00	612	+17·77	+ 1·08	+19·19	+49·89	+30·70	$+41{\cdot}5 \times 10^{25}$
00	720	+11·09	+ 1·35	+14·97	+30·11	+15·14	
50	693	+ 5·18	+ 1·24	+ 6·42	+17·31	+10·79	
00	356	+ 0·22	+ 0·32	+ 0·07	+ 5·04	+ 4·97	
			GROUP III $\bar{\phi}$ = 45°N.				
00	43	+13·07	− 2·79	−36·47	−29·95	+ 6·52	
00	125	+20·00	− 0·89	−17·80	+ 6·61	+24·41	
00	225	+18·40	− 1·23	−22·63	− 4·93	+17·70	
00	448	+16·34	− 2·46	−40·20	−35·18	+ 5·02	$+8{\cdot}4 \times 10^{25}$
00	596	+11·35	− 1·23	−13·96	−13·89	+ 0·07	
50	553	+ 6·30	− 0·23	− 1·45	− 0·17	+ 1·28	
00	140	+ 1·41	− 0·60	− 0·85	− 2·61	− 1·76	
			GROUP IV $\bar{\phi}$ = 55°N.				
00	17	+16·94	+ 0·88	+14·91	+11·29	− 3·62	
00	89	+16·18	− 0·44	− 7·12	−46·92	−39·80	
00	155	+17·61	+ 1·17	+20·60	− 0·92	−21·52	
00	268	+13·82	− 0·62	− 8·57	− 8·91	− 0·34	$-6{\cdot}2 \times 10^{25}$
00	285	+ 7·78	+ 0·06	+ 0·47	+ 5·47	+ 5·00	
50	289	+ 6·51	+ 0·37	+ 2·41	− 0·90	− 3·31	
00	140	+ 3·16	− 0·66	− 2·09	− 4·71	− 2·62	
			GROUP V $\bar{\phi}$ = 63°N.				
00	6	+34·67	0·00	0·00	+54·83	+54·83	
00	92	+12·41	+ 2·65	+43·49	+ 8·08	−35·41	
00	135	+12·62	+ 1·73	+21·83	+27·30	+ 5·47	
00	206	+ 8·48	+ 0·34	+ 2·88	−10·86	−13·74	$-2{\cdot}0 \times 10^{25}$
00	228	+ 5·02	+ 0·77	+ 3·87	+ 1·78	− 2·09	
50	228	+ 3·21	+ 0·57	+ 1·83	− 5·05	− 6·88	
00	91	− 7·14	+ 0·13	− 0·93	+ 4·18	+ 5·11	
			GROUP VI $\bar{\phi}$ = 76°N.				
00	3	+18·33	− 4·67	−85·60	−112·00	−26·50	
00	31	+ 6·58	− 0·19	− 1·25	−21·61	−20·36	
00	89	− 2·11	− 1·73	+ 3·65	+21·67	+18·02	
00	138	− 4·25	− 2·76	+11·73	+26·61	+14·88	$+0{\cdot}6 \times 10^{25}$
00	154	− 1·62	− 1·55	+ 2·51	+10·19	+ 7·68	
50	151	− 0·84	− 0·45	+ 0·38	+ 1·48	+ 1·10	
00	101	− 1·35	− 0·28	+ 0·38	− 1·46	− 1·84	

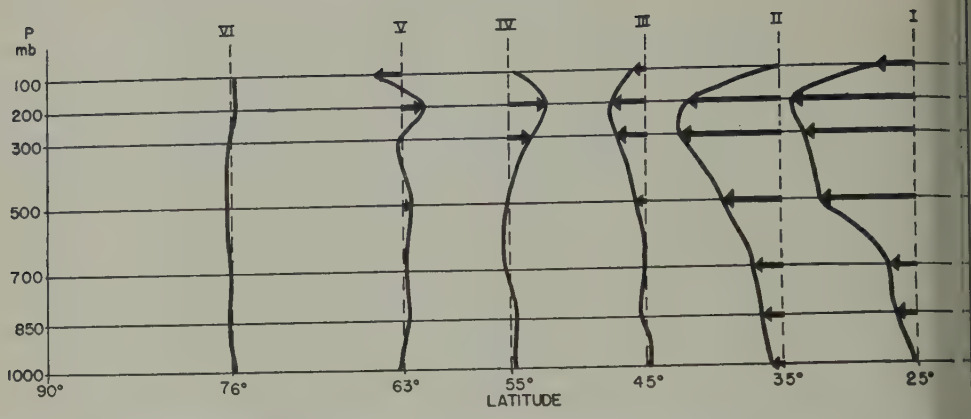

Figure 2. Profiles of angular momentum transport vectors for complete latitude circles. Transports are for layers of unit mass in the vertical. It should be noted that the profiles would be much altered if elevation were used as the vertical coordinate and the transports given for layers of unit geometrical thickness.

explain so large a discrepancy, if one were to assume that the computations presented here are correct.

(1) The North American sector of the northern hemisphere is characterized by better developed eddies than, let us say, the Eurasian sector and hence the values obtained are not representative of the hemisphere.

(2) The month of February 1949 is an abnormal February. That departures from normal as large as this could exist is easily possible.

(3) The estimates of normal surface torques used is too small.

(4) Angular momentum is transferred also by meridional cells. A direct cell in the troposphere would however, add to to the transport and make matters worse.

Of these four possibilities (1) and (3) might be the most plausible, since examination of hemispheric conditions during February 1949 does not show too striking abnormalities, and no good reason suggests itself for recourse to (4).

The table and diagram suggest the presence of very strong eddy transports at the jet-stream level as might perhaps be expected on general grounds from the violence of the winds in this region*. If any reliance can be placed upon the results at the 100 mb level, the sharp drop in the transports (except for Group V) shown here is most remarkable. It should be remembered that the effects of selectivity at this level are most severe, however. Generally speaking the meridional gradients of the transports are what should be expected from friction and other surface effects, except for the decrease from 25°N. to 35°N.

The most that can be said for the results is that they suggest the presence of strong eddy transfers of momentum, as proposed by the writer elsewhere (Starr 1948). The need for comprehensive treatment of the subject is apparent.

* NOTE ADDED IN PROOF.—The writer's colleague, Dr. H. L. Kuo, has pointed out that on hydrodynamical grounds the meridonal profile of angular momentum flow obtained for these higher levels, when considered in relation to the mean zonal wind profiles, suggest the presence of a transfer of existing kinetic energy from the large-scale perturbations to the mean zonal motion.

Acknowledgment

I wish to thank Mr. Kendall Peterson for performing the computations reported this note.

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Advective temperature change in the 1,000-700 mb and 700-500 mb layers

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(Manuscript received 6 May 1950, in revised form 4 August 1950)

SUMMARY

It is shown, on the basis of about 700 measurements made at a particular station during six ıths, that the thermal change in the troposphere due to advection alone is largely offset by the ır processes at work, so that the resultant change is a residual having a correlation of about 0·6 ı the advective component.

The shear hodograph of an upper wind sounding is shown to afford little useful information on resultant thickness tendency.

1. INTRODUCTION

The local rate of change of thickness between chosen isobaric surfaces can be olved mathematically into three components, termed *advective*, *dynamical* and *-adiabatic* (Sutcliffe and Forsdyke 1950). The advective component is the iest to estimate, qualitatively, from the synoptic chart, and it is of both theoretical l practical interest to know how far and in what way is the resultant thickness dency determined by advective processes. This paper sets out to answer this stion, not in specially chosen situations, but in the general run of weather such may be experienced at one station over a considerable period of time, by measuring advective component and the resultant tendency on a large number of occasions l finding the statistical relationship between them. Similar treatment is given the *hodograph* component, defined below, which at first sight is a reasonable asure of the advective component. The range of possible measurements is erely limited by the comparative sparseness of the network of upper air sounding ions, and by the requirements that the method of measurement should be able of being used in any synoptic situation but not be so laborious as to preclude examination of a large amount of data. Quantities which cannot be measured e to be estimated from those which can, and if the results are to have physical hificance, and to be independent of the methods of estimation used, it is abso-ely necessary that these methods should be tested and proved adequate by some ective standard. A suitable standard, the *relative residual variance* (denoted by S), lefined in the Appendix and its use illustrated. All the methods of estimation d have been proved adequate by this standard, but the work, which is tedious l of little meteorological interest, has been excluded from the present paper. s contained in a more extensive paper which has been deposited with the Society l is available for inspection.

Using the notation of Sutcliffe and Forsdyke, with the addition that $\mathrm{grad}_p T$ otes the temperature variation in the (nearly horizontal) isobaric surface, the ective component may be written

$$A = -\frac{R}{g}\int_{p}^{p_0} \mathbf{v}\,\mathrm{grad}_p T\, d\log f \qquad . \qquad . \qquad (1)$$

On the assumption, reasonable if the layer considered is not too deep, that $\text{grad}_p T$ is constant throughout the layer, this becomes

$$A = -\frac{R}{g}\text{grad}_p T \int_p^{p_0} \mathbf{v}\, d\log p \quad . \quad . \quad . \quad . \quad (2)$$

Eq. (1) may be modified by substitution from the "thermal wind" equation

$$\frac{\partial}{\partial p}\left(\frac{d\mathbf{v}}{dt}\right) + l\mathbf{k} \times \frac{\partial \mathbf{v}}{\partial p} = \frac{R}{p}\text{grad}_p T \quad . \quad . \quad . \quad (3)$$

and becomes

$$A = \frac{l}{g}\mathbf{k}\int_p^{p_0} \mathbf{v} \times \frac{\partial \mathbf{v}}{\partial p}\, dp - \frac{1}{g}\int_p^{p_0} \mathbf{v} \cdot \frac{\partial}{\partial p}\left(\frac{d\mathbf{v}}{dt}\right) dp \quad . \quad . \quad (4)$$

If the second term on the right hand side of Eq. (4) vanishes, which it does if the ageostrophic component of the airflow is invariant with height, then the advective component is given by the first term. This is proportional to the area enclosed on a polar diagram between the wind vectors for levels p_0 and p and the hodograph of the wind vector for the layer between, and will be called the *hodograph* component.

2 The data

The advective component for the 1,000-700 mb and 700-500 mb layers at Downham Market was measured by means of Eq. (2) at six-hourly intervals for six consecutive winter months. The value of $\text{grad}_p T$ was estimated from the thickness lines on the charts analysed at the Central Forecasting Office, and, including the proper factor of proportionality, was multiplied by the component of $\int_p^{p_0} \mathbf{v}\, d\log p$ in the direction of $\text{grad}_p T$ found from the upper wind sounding. Values of the actual thickness at Downham Market at the same hours, computed from the temperature soundings, were taken from the same charts and used to estimate the resultant thickness tendency. The raw data were used to estimate the mean advective component and resultant thickness tendency over a period of six hours

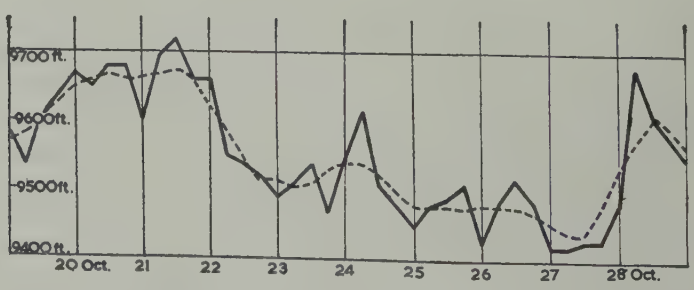

Figure 1. A comparison between the series of observed thicknesses of the 1,000-700 mb layer for Downham Market, October 20 to 28 1945 (full line) with the derived series (pecked line) obtained by means of the smoothing formulae. The derived series follows the main features but not the small irregularities of the original.

etween synoptic hours. The processes of estimation were chosen carefully to ve quantities which were properly comparable and representative of the large-ale thermal changes in the troposphere, while eliminating the diurnal temperature riation and as far as possible the random observational error. Their effectiveness, hich is illustrated in Fig. 1, was tested in the way shown in the Appendix.

The hodograph component was tested in similar fashion using three years' data, o for Downham Market and one for Larkhill.

3. Discussion of the results

(a) *The advective component*

The correlations between the thickness tendency and the advective component Downham Market during the six months October 1945 to March 1946 are given Table I.

TABLE I. Correlations between thickness tendency and advective component at Downham Market during the months Oct. 1945 to Mar. 1946.

Month	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Total
No. of obs.	122	120	124	124	112	124	726
1,000-700 mb	0·54	0·59	0·56	0·43	0·60	0·69	0·64
700-500 mb	0·60	0·79	0·41	0·53	0·70	0·46	0·53

Using the correlations for the whole series of data, the regression of the thickness ndency on the advective component is

$$t = -0{\cdot}36 + 0{\cdot}54\,a\ ;\ \text{for the 1,000-700 mb layer} \quad . \quad . \quad . \quad (5)$$

$$t = -0{\cdot}33 + 0{\cdot}39\,a\ ;\ \text{for the 700-500 mb layer} \quad . \quad . \quad . \quad (6)$$

These regression equations give the best estimates of the thickness tendency hich can be made on the basis of the advective component. They show that ie advective component has on average a positive bias compared with the thickness ndency, so that during that winter over the SE. of the British Isles advection ad the overall character of a warming process, while the non-advective processes ad an overall cooling effect.

The reliability of these estimates is poor, with $S = 0{\cdot}59$ and $0{\cdot}72$ respectively, ut they may sometimes be the only means of estimating the thickness tendency. so, the equations themselves may be used, but, as shown in the Appendix, very ttle will be lost if a close approximation is used instead. For practical purposes is recommended that where there is no other evidence on the subject, the thickness ndency should be taken to be half the advective component. It has sometimes een suggested that the thickness tendency should be put equal to the advective mponent, but it is easy to show, by finding S, that such an estimate is almost orthless.

(b) *The non-advective component*

The sum of the dynamical and non-adiabatic components, which may be termed the non-advective component, may be found by subtracting the advective component from the thickness tendency. The correlations of this with the thickness tendency and with the advective component are given in Tables II and III.

TABLE II. Correlations between thickness tendency and non-advective component at Downham Market during the months of Oct. 1945 to Mar. 1946.

Month	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Total
No. of obs.	122	120	124	124	112	124	726
1,000-700 mb	0·34	0·35	0·26	0·33	0·42	0·36	0·35
700-500 mb	0·24	0·14	0·30	0·08	0·17	0·54	0·22

TABLE III. Correlations between advective and non-advective components at Downham Market during the months Oct. 1945 to Mar. 1946.

Month	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Total
No. of obs.	122	120	124	124	112	124	726
1,000-700 mb	−0·60	−0·55	−0·67	−0·71	−0·47	−0·42	−0·63
700-500 mb	−0·64	−0·72	−0·75	−0·81	−0·58	−0·50	−0·71

The negative correlation between advective and non-advective components is noteworthy and supports the impression, gained by a study of synoptic charts, that the effect of advection in modifying the thermal field is largely offset by the other processes at work. It appears, moreover, that the compensation is generally more complete in the 700-500 mb layer than it is in the 1,000-700 mb layer, since in each of the six months the negative correlation is larger for the upper layer than it is for the lower.

(c) *The hodograph component*

The hodograph component is equal to the advective component if the airflow is geostrophic or if the ageostrophic wind does not vary with height. Otherwise it forms only an approximation to the advective component, but even so, it has received wide publicity as affording a reasonable approximation to the thickness tendency itself, for use in practical forecasting. (See *e.g.* Jones (1943), Miller and Thompson (1942), Rossby, Oliver and Boyden (1943) or Starr (1942).

A comparison between the hodograph component and the thermal tendency for the layer between 5,000 and 10,000 ft has been carried out by Panofsky (1944) in which he obtains a correlation of 0·50, based on 86 pairs of observations, the hodographs being obtained from pilot balloon soundings. This result does not conflict with those obtained in the present more extensive investigation, in which about 4,000 pairs of observations were used, which are summarised in Table IV.

ABLE IV. DATA ON THE HODOGRAPH COMPONENT AND ITS STATISTICAL RELATION TO THE THICKNESS TENDENCY FOR 6 HR INTERVAL

ation	Larkhill		Downham Market			
riod	Oct. 47—Mar. 48	Apr. 48—Sept. 48	Oct. 45—Mar. 46	Apr. 46—Sept. 46	Oct. 47—Mar. 48	Apr. 48—Sept. 48
o. of obs. (approx.)	730	729	725	500	590	725
000-700 mb						
Mean tendency (ft)	— 1·5	— 0·4	— 1·0	— 1·1	— 0·1	0·3
S.D. (ft)	25·6	22·0	22·2	24·9	27·9	22·2
Mean hodograph component (ft)	36·7	12·8	21·6	18·6	37·0	14·8
S.D. (ft)	53·4	33·9	53·3	35·7	54·0	31·0
Correlation	0·440	0·446	0·104	0·391	0·209	0·414
Regression coefft.	0·21	0·29	0·04	0·27	0·11	0·29
0-500 mb						
Mean tendency (ft)	— 0·5	— 0·3	— 1·2	— 0·8	— 0·6	0·5
S.D. (ft)	27·9	24·5	27·5	22·0	30·8	24·1
Mean hodograph component (ft)	8·7	— 0·4	0·5	3·3	10·7	2·2
S.D. (ft)	53·9	33·4	41·4	32·6	46·5	29·4
Correlation	0·458	0·573	0·200	0·504	0·495	0·454
Regression coefft.	0·24	0·42	0·13	0·34	0·32	0·37

It appears that the hodograph component is much more variable than the ickness tendency, and in the 1,000-700 mb layer has a large positive bias, especially uring the winter half-year. The regression coefficients by which the departure the hodograph component from its mean must be multiplied to give the best timate of the thickness tendency are also given in Table IV. They vary a good al from one half year to another, but the three years' data suggest that if measure-ents were continued indefinitely, the mean regression coefficient would be perhaps 20 for the 1,000-700 mb layer and 0·30 for the 700-500 mb layer. If, however, regression equation using the long-term regression coefficient is used in fore-sting, it will lack precision when the results are considered over a six-months' eriod not only because of the low correlation, but also because the long-term gression coefficient differs from the six-months' value. If the value of S is worked t for each period using the long-term regression coefficient (the only value available fore the event) it will be found nearly always to exceed 0·9 and sometimes to ceed unity. Hence we conclude that no statistical estimate based only on the easured hodograph component will provide a worthwhile amount of information to the thickness tendency.

In the American work already quoted, the hodograph component is used to btain the correction for temperature change in an air column to enable the pressure ndency at 10,000 ft to be obtained from the surface pressure tendency. This is quivalent to using the inexact regression equation which puts the thickness tendency qual to the hodograph component instead of that embodying the long-term gression coefficient. It may be shown that the relative residual variance of this timate is greater than unity for each of the six-months' periods here considered, that this adjustment to the pressure tendency introduces on average larger errors

than those involved in ignoring temperature change altogether. This conclusion does not rule out the possibility that the hodograph component may give useful information on the thickness tendency in special situations (which a synoptic meteorologist may be able to recognise) or that it may be useful in conjunction with other data, but is a powerful argument against its indiscriminate or unthinking use.

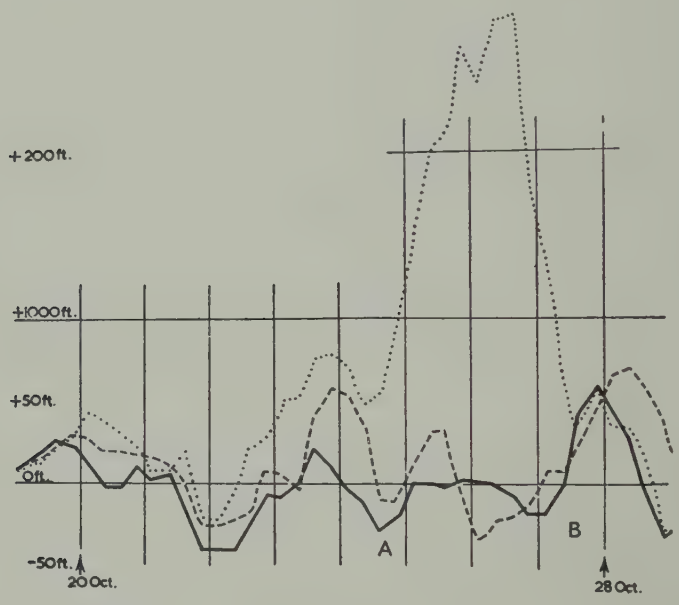

Figure 2. Corresponding values of the thickness tendency (full line), the advective component (pecked line) and the hodograph component (dotted line) for Downham Market for the 1,000-700 mb layer for 20 to 28 October 1945. There is a general similarity between the curves except in the section AB, where the hodograph component differs widely from the others.

An examination of graphs on which thickness tendency and hodograph component are plotted side by side (Fig. 2) shows that the correlation between the two is not uniformly bad, but that there are periods of time, ranging from 12 hr to six days, for which the correlation is noticeably worse than at other times. These periods are characterised in the 1,000-700 mb layer by a large positive hodograph component and a much smaller, often negative thickness tendency. Their existence shows that the indications of the hodograph component, or qualitatively, of the wind veering or backing with height, are most suspect when they are most striking. A wind backing or veering slightly with height may throw some light on the probable thermal change, whereas a large veering throws none.

4. Conclusion

Judging by the data here used, the advective component, taken by itself, does not afford a satisfactory basis for estimation of the thickness tendency. On the

trary, changes due to advection are largely offset by those due to dynamic heating cooling and by non-adiabatic processes, so that the resultant thermal change ears as a small residual. These conclusions apply even more strongly to the ograph component, which taken by itself offers very little information as to thickness tendency. Even that little is not a constant, but varies a good deal n one six-monthly period to another.

In current synoptic practice in the British Meteorological Office, a forecast kness chart is based partly on an estimation of the effect of advection and the er physical processes in changing the thickness pattern and partly on empirical wledge of the behaviour of thickness patterns. The present work does not alidate such estimates of thickness change, into which many considerations enter, it does show that satisfactory prognostic charts cannot be constructed on the is of advection alone. The need for investigating the non-advective processes clearly underlined.

Acknowledgment

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Appendix

a) *The relative residual variance, used as a measure of the reliability of estimates*

The meteorologist often has to make use of estimates for quantities of which true value is known after the event. One problem of estimation arises when, example, he is called on to forecast the night minimum temperature, while ther, that of statistical estimation, is exemplified when he uses an observed te difference in place of a differential coefficient, itself difficult or impossible measure in the time available, the value of which is required as basis for further culations. The present note offers a method whereby the reliability of any thod of making such estimates can be determined objectively, provided only t the method has been applied a reasonable number of times.

Suppose that u, the required variable, has been measured on n occasions and that e is the estimate found on each occasion by the proposed method. The following quantities can be found without difficulty :—

the mean values $\quad \bar{u} = \frac{1}{n}\Sigma u, \quad \bar{e} = \frac{1}{n}\Sigma e$

the variances $\quad \sigma_u^2 = \frac{1}{n}\Sigma(u-\bar{u})^2, \quad \sigma_e^2 = \frac{1}{n}\Sigma(e-\bar{e})^2$

and the correlation $\quad r_{ue} = \frac{\Sigma(u-\bar{u})(e-\bar{e})}{\{\Sigma(u-\bar{u})^2\,\Sigma(e-\bar{e})^2\}^{\frac{1}{2}}}$

A familiar measure of the discrepancy between observation and estimate is the mean square departure, or residual variance $1/n\,\Sigma(u-e)^2$ which can be reduced to the dimensions of a pure number by dividing by $1/n\,\Sigma(u-\bar{u})^2$. This ratio, $S = \Sigma(u-e)^2/\Sigma(u-\bar{u})^2$ is the relative residual variance, which is our proposed means of judging the reliability of estimates. For a perfect method of estimation $S = 0$ while for any other S is positive. Of two imperfect methods that with the lower value of S is usually the better (the exceptions are cases, which sometimes arise in meteorology, where some errors are more important than others, so that the mean square error is not what we wish to minimise).

For the simple method of estimation which consists in using $\bar{u}$, the mean value, on every occasion, $S = 1$, and this will, of course, be used in preference to any method for which $S > 1$. As a rough scale of reliability which nevertheless corresponds fairly well with the general usage of the terms in meteorological work, we may suggest that given in Table V.

TABLE V. SCALE FOR JUDGING THE REALIABILITY OF ESTIMATES

S, the relative residual variance	the lowest correlation between observed and estimated values for which the given value of S is possible	Description of the reliability of the method of estimation
0·1	0·95	very good
0·2	0·9	good
0·5	0·7	moderate
0·7	0·55	poor
0·9	0·3	almost worthless

Consider now a new estimate e' obtained from the original one by the linear transformation $e' = be + c$. For this estimate we have

$$S = \frac{\Sigma(u-e')^2}{\Sigma(u-\bar{u})^2} = 1 - r_{ue}^2 + \left(r_{ue} - \frac{b\sigma_e}{\sigma_u}\right)^2 + \left(\frac{\bar{u}-b\bar{e}-c}{\sigma_u}\right)^2 \quad . \quad . \quad (7)$$

This expression shows that the least value of S, involving only the correlation ween u and e, is obtained only when $b = r_{ue}\sigma_u/\sigma_e$ and $c = \bar{u} - b\bar{e}$. In other ds, the greatest possible reliability is obtained only if the original estimate is sformed to give what is in fact the linear regression of u on e. Any other linear sformation, and the original estimate itself, will have lower reliability, the loss g measured by the essentially positive terms $(r_{ue} - b\sigma_e/\sigma_u)^2$ and $(\bar{u} - b\bar{e} - c)^2/\sigma_u^2$ point is important, because an investigator who has produced a method of mation founded on reasonable physical arguments may easily overlook the need such linear transformation, particularly if he finds a high correlation between estimate and observation. In fact, a high correlation is no guarantee that the mate is good, but simply that the right linear transformation will make it good.

In practice, we cannot give b and c their best values, for they may have to be nded to values convenient for calculation and in any event can only be estimated n an experimental sample which cannot be fully representative of the population n which it is drawn. The final term $(\bar{u} - b\bar{e} - c)^2/\sigma_u^2$ is unimportant in the present k, and will not be further discussed, but we must consider how far the para- er b may differ from its best value without the reliability of the estimate being busly impaired. Any value which makes $r_{ue} - b\sigma_e/\sigma_u$ equal to or less than 0·1 be acceptable (for the contribution of $(r_{ue} - b\sigma_e/\sigma_u)^2$ to S will then be quite ligible), while with estimates of moderate reliability a value as high as 0·3 for $-b\sigma_e/\sigma_u$ may sometimes be acceptable. This gives us a range within which we choose any convenient value of b.

If the value actually chosen for b lies so far outside this range that $-b\sigma_e/\sigma_u)^2 > r_{ue}^2$, then $S > 1$, and the method of estimation is worse than useless. particular, the original estimate e, for which $b = 1$, is worse than useless if

$$r_{ue}\,\sigma_u/\sigma_e < \tfrac{1}{2} \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

s result makes it clear that any method of estimation which, like so many in eorology, is based on reasonable but not indisputable physical arguments must, re it can be accepted unreservedly, be verified statistically. It must be shown, the calculation of S from an adequate body of data, that the values chosen for parameters b and c are sufficiently near the optimum values. The use of the ograph component in the American works already quoted shows that in the ence of such verification a plausible " correction " may in fact introduce errors er than those which it removes.

The relative residual variance was also used in the present work in solving problems of statistical estimation. These are set out in full in the longer paper ch has been deposited with the Society ; only the following illustrative example iven here.

(b) *Estimation of the mean advective component*

The notation is that $a_1\ a_2, \ldots . a_n$ refer to observations at synoptic hours r apart, and that $r_1\ r_2$ *etc.* are the lag correlations between observations, 6, 12 hr apart. The advective component $a_{n+1/2}$, which actually has been obtained n the raw data by smoothing with a four-term moving average, refers to a single ant of time, and must be used to estimate something comparable with the

measured thickness tendency, which is essentially an average value over a period of 6 hr. In fact, we seek the best estimate of the form $k a_{n+1/2}$, (where k is an arbitrary parameter) for the value of $\int_n^{n+1} a_{n+c}\,dc$.

It may be shown that for minimum residual variance

$$k = 2\int_0^{\frac{1}{2}} r_c\,dc \qquad . \quad . \quad . \quad . \quad . \quad (9)$$

and that the relative residual variance is given by

$$S = 1 - \frac{2\left\{\int_0^{\frac{1}{2}} r_c\,dc\right\}^2}{1 - \int_0^1 c\,r_c\,dc} \qquad . \quad . \quad . \quad . \quad (10)$$

Now these expressions contain r_c, the lag correlation coefficient, with fractional suffixes, whereas this coefficient can only be observed for integral values of the suffix. Nevertheless, the integral values, which can be calculated, follow one another smoothly for all the data used here, and afford reasonable grounds for interpolation. Actually, neither the fractional power law suggested by Sutton (1932) nor the exponential law advocated by Schumann (1941) would give a satisfactory fit for the present data, and a linear form, which if anything underestimates the correlation for small c, has been used.

$$r_c = 1 - c(1 - r_1) \qquad . \quad . \quad . \quad . \quad . \quad (11)$$

Inserting this in Eqs. (9) and (10) gives $k = (3 + r_1)/4$ and

$$S = (53 - 50r_1 - 3r_1^2)/16(5 - 2r_1)$$

Now, in the series used, the values of r_1 range from 0.80 to 0.93, so that k is always very nearly unity. We can in fact replace k by unity without any material loss in precision, for the second term in Eq. (7) will be negligible, and S will have a value of 0·2 or less. Thus the method of estimation which consists in using $a_{n+1/2}$ as our estimate for $\int_n^{n+1} a_{n+c}\,dc$ should be judged good by the standard set out in Table V.

All the formulae of estimation used have been tested and found good, and it can be affirmed with some confidence that the correlations given in the main paper are inherent in the data and not in any way a product of the processes of estimation.

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vertical convective heat flux in the atmosphere—a critical essay

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SUMMARY

A definition of convective vertical heat flux is proposed which leads directly to the expression $\overline{\rho_1 w_1 h_1}$ where ρ_1, w_1, and h_1 are, respectively, simultaneous values of the fluctuating density, vertical velocity relative to the surface, and total heat per unit mass at a point in a constant pressure surface. An assumption (apparently justifiable) of the pressure fluctuations yields the expression $c_p p \overline{w_1}/R$ for the convective flux of sensible heat. The distinction between free and forced convection is made. Recent statements by Montgomery and by Priestley and Swinbank are examined with particular attention to the importance of free convection. It is submitted that neither Montgomery's neglect of the buoyancy forces nor Priestley and Swinbank's arguments suggesting their universal importance can at present be justified. In particular the latter authors' explanation of the normal tropospheric lapse rate is questioned.

1. INTRODUCTION

Convective transfer of heat in the atmosphere may be loosely defined as transfer involving the movement of air from place to place. This discussion will be confined with convection in a more limited sense — transfer in the vertical by processes operative over a horizontal area of the order of not more than tens of miles *i.e.* excluding those processes, involving quasi-horizontal motion associated with major atmospheric pressure systems, usually described as dynamical. Some precision may be given to these ideas by considering the mean value of the heat flux over the whole area of a system comprising a section of the atmosphere bounded by vertical material walls. Advective and dynamical effects are thus excluded.

Within these limitations convective processes fall into two classes, free and forced. Heat transfer by free convection results from motion of air produced by buoyancy forces occurring within the air mass as a result of the imposed temperature differences. There being no external source of kinetic energy, free convection can only reduce the potential energy of the system in which it occurs. It is not necessarily restricted to transfer of heat from levels of high to levels of low *mean* temperature in the system defined above. It is obvious that any convective process within a system involves horizontal inhomogeneity of temperature. Heat transfer by forced convection results from motion of air produced by forces external to the system considered. There is therefore no necessary tendency to minimize the potential energy of the system ; in the atmosphere the process will usually tend to increase potential energy. The transfer of heat is down the gradient of potential temperature, *i.e.* not necessarily from high to low temperature ; in fact in the normal atmospheric case it is from regions of low to regions of high temperature. Since convective flux of heat involves inhomogeneities of temperature there is an element of free convection in all cases of forced convection. In what follows forced convection will be used to designate cases in which the work done on the system

by external forces is much greater than that done by the buoyancy forces. In some atmospheric processes, it is by no means obvious which form of convection makes the greater contribution.

The classical meteorological treatment of vertical convective heat transfer, originated by Taylor and Schmidt (Brunt 1939) refers to forced convection only, since it is assumed that the correlation between the temperature of an element of air at rest in a given level and its subsequent vertical velocity is negligible. Ertel saw the importance of this restriction, but his discussion appears to have been incomplete (for references to Ertel's work and subsequent controversy see Van Mieghem 1949, p. 56). Priestley and Swinbank (1947) studied the problem, and advanced the thesis that the significance of free convection is equal to or greater than that of forced convection in atmospheric processes, basing their arguments mainly on synoptic grounds. Montgomery (1948) reformulated the problem, and produced expressions for the convective heat flux in apparent agreement with the Taylor-Brunt formula. The purpose of this paper is to submit the arguments of Priestley and Swinbank, and certain aspects of Montgomery's work, to critical examination. It will be shown that Montgomery has neglected the buoyancy forces which lead to free convection, but it will also be proposed that the arguments of Priestley and Swinbank are in some ways ill founded and are inconclusive in deciding the relative importance of free and forced convection in the atmosphere.

2. Definition of the vertical convective flux

We are interested in the mean value of the heat flux over the whole area of a system comprising a section of the atmosphere bounded by vertical material walls. In such a system there is no mean mass transport through a constant pressure surface, and in such a surface we will denote the mean value of density, temperature, total heat per unit mass *etc.*, by ρ, T, h, *etc.* The instantaneous values of these quantities in an element of area ds and height dz about the constant pressure surface will be denoted by ρ_1, T_1, h_1, *etc*, and the instantaneous vertical component of velocity of the air within the element relative to the level by w_1. The quantity of heat transported through the level by the air in such an element will be *defined* as the amount of heat which would be made available if the element, having been arrested in the level, were brought at constant pressure to the mean state of the air at this level (but see the proviso below).

For the element considered this quantity is

$$\rho_1 \, ds \, dz \, (h_1 - h) \qquad . \quad . \quad . \quad . \quad . \quad (1)^*$$

The heat flux due to this element is

$$\rho_1 \, w_1 \, (h_1 - h)$$

and the mean flux due to all elements

$$\overline{\rho_1 \, w_1 \, (h_1 - h)} \qquad . \quad . \quad . \quad . \quad . \quad (2)$$

$$= \overline{\rho_1 w_1 h_1} - h \, \overline{\rho_1 w_1} = \overline{\rho_1 w_1 h_1} \qquad . \quad . \quad . \quad . \quad (3)$$

* In the usual notation

$$h = u + pv : \; dh = du + p\,dv + v\,dp = du + p\,dv(p \text{ const}) = dq.$$

e $\overline{\rho_1 w_1}$ is the mean rate of mass transport which is zero in the constant pressure l. In dry air, or with negligible error in moist unsaturated air, the mean flux ensible heat is

$$\overline{c_p \rho_1 w_1 (T_1 - T)} = \overline{c_p \rho_1 w_1 T_1} \quad . \quad . \quad . \quad (4)$$

shall be mainly concerned with this expression.

Expression (3) has been derived by Montgomery (1948), and (2) by Van ghem (1949), using different methods of approach. Neither of these derivations s on any hypothesis concerning the nature or cause of the local fluctuations of sity, specific total heat, or vertical wind. Both do however make it clear that e is another possible mechanism of vertical heat transport, by degradation to energy of the kinetic energy of the moving elements, a possibility ignored in definition on which (1) is based. Heat transport would occur in this way if e were a systematic flux of kinetic energy of turbulent motion through the level. as not been demonstrated that the effect is significant. Another point to note at our definition tacitly ignores the question of pressure fluctuations by supposing element to be brought to the mean state of its surroundings *at constant pressure.* n Van Mieghem and Montgomery imply this, by writing $\rho_1 h_1 = \rho_1 u_1 + p$. ler (1949) quotes an anlysis by G. I. Taylor which demonstrates that in a natural l the relative fluctuations of pressure are of the order of $10^{-3}\times$ those of tempera-. It is therefore permissible to write

$$p = R \rho_1 T_1$$

(4) reduces to

$$c_p p \overline{w_1}/R \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

s shows that the flux of heat through a constant pressure level is in the direction he Reynolds mean of the vertical velocities relative to the level and, since $\overline{\rho_1} = 0$, in the direction of motion of the less dense but hotter elements. It lear therefore that in heat flow due only to density variations in the field of ity (free convection) the heat flow will always be directed upwards, whatever direction of the mean temperature gradient, whilst in forced motion of air pendent of density variations the heat flow will always be down the mean lient of potential temperature, whatever the direction of the temperature gradient. ression (5) also makes it clear that the vertical transport of heat by free convection ot simply a matter of the upward motion of those elements with greater heat ent as is sometimes loosely stated. The amount of heat transported through a stant pressure level *per unit volume* by an element is independent of the tem-ture of the element, but the rate of heat transport is greater for the faster moving nents.

The relation between convective heat flux and temperature gradient

We shall be concerned in this section with the convective flux of sensible heat nsaturated air, *i.e.* with (4) or (5). These expressions are of little practical in estimating the heat flux, owing to the difficulty of measuring the fluctuations emperature and wind. Attempts have therefore been made, by analogy with molecular conduction of heat, to relate the heat flux to the temperature gradient.

The classical Taylor-Brunt method employs the mixing length concept, and as pointed out in section 1, neglects the effect of free convection. The method leads to the expression

$$c_p \overline{\rho_1 w_1 T_1} = -\rho c_p K(\partial T/\partial z + \Gamma) \quad . \quad . \quad . \quad (6)$$

where, by the arguments of section 2, K is essentially a positive quantity. Priestley and Swinbank, maintaining that the neglect of free convection cannot be justified, and employing a modification of the mixing length concept, produce an expression of the form of (6) in which K is able to assume positive and negative values.

Montgomery attacks the problem from a different standpoint. He *defines the (kinematic) eddy diffusivity* K by the equation

$$\overline{\rho_1 w_1 s_1} = -K\bar{\rho}_1 \partial s/\partial z \text{ (our notation)} \quad . \quad . \quad (7)$$

where s is the measure of any property unchanged by simple transportation to a different level. He establishes (3) by thermodynamic arguments. He then introduces a function Y, the sum of total heat per unit mass and geopotential

$$Y = h + \phi$$

and considers a vertical adiabatic displacement, through an elementary pressure range dp, of an element of air, initially of specific total heat h_1 and specific volume α_1 in surroundings of specific volume α. Then Montgomery says :

$$\text{“ } dY = dh + d\phi = (\alpha_1 - \alpha)\, dp$$

Therefore, except for the negligible change due to the difference in specific volume between parcel and environment, the function Y is unchanged by simple vertical displacement of the parcel.” Eq. (7) then becomes

$$\overline{\rho_1 w_1 Y_1} = \overline{\rho_1 w_1 h_1} = -K\bar{\rho}_1 \partial Y/\partial z \quad . \quad . \quad . \quad (8)$$

which leads, for the transport of sensible heat in unsaturated air, to (6).

It is, however, clear that the neglected factor $(\alpha_1 - \alpha)\, dp$ is precisely the work done by the buoyancy forces in the displacement considered and that to neglect it is to neglect these forces. In this respect therefore Montgomery's treatment shows no advance on the Taylor-Brunt method — he has replaced the somewhat unsatisfactory “ mixing length ” deduction of the relation between convective heat flux and potential-temperature gradient by a straightforward assumption of proportionality but has been obliged to neglect the buoyancy forces in applying this method to heat transfer. His analysis does in fact show that heat transfer by free convection through a constant pressure surface in the system considered is not a linear function of the mean vertical gradient of potential temperature in the surface. (This conclusion is analogous to that reached in studies of the convective heat loss from hot bodies. Application of dimensional methods to this problem shows that heat loss by forced convection is a linear function of the temperature excess of the body but that the relation is more complex in the case of free convection). On these grounds Priestley and Swinbank's attempt to reconcile the Taylor-Brunt type of formula with the existence of the buoyancy forces seems unprofitable.

4. The total flux of heat in moist air

In this section, a digression from the main theme of the paper, certain statements of Montgomery concerning the total convective flux of heat in moist air will be considered. The following notation will be used : the isobaric specific heat of dry air ${}_dc_p$, of moist air of water content e per unit mass ${}_mc_p$, of water vapour ${}_{wv}c_p$, and of liquid water ${}_wc_p$; the total heat per unit mass of these substances at temperature T_a — ${}_dh_a$, ${}_mh_a$, *etc.*, and at temperature T_b — ${}_dh_b$, ${}_mh_b$, etc. ; the latent heat of vapourisation of water at T_a, T_b — λ_a, λ_b. The total heat per unit mass of moist air at temperature T_b is

$$\begin{aligned} {}_mh_b &= (1-e)_dh_a + e_wh_a + \lambda_a e + {}_dc_p(1-e)(T_b - T_a + {}_{wv}c_p\, e(T_b - T_a) \\ &= (1-e)_dh_a + e_wh_a + \lambda_a e + {}_mc_p(T_b - T_a) \qquad . \quad . \quad . \quad . \quad (9) \end{aligned}$$

Montgomery assumes the arbitrary total heat at the reference temperature T_a to be zero, and writes for the total heat per unit mass of moist air at T_b

$$ {}_mh_b = \lambda_a e + {}_mc_p(T_b - T_a) \qquad . \quad . \quad . \quad . \quad (10)$$

He then says : " It is significant that the heat flux $\overline{\rho_1 w_1 h_1}$ depends on the arbitrary quantity T_a. In general the eddy flux of heat cannot be specified absolutely in terms of local phenomena (w_1, T_1, e_1, *etc*) alone." This appears to be fallacious : the total heat per unit mass is a single-valued function of state and does not depend on the history of the system considered. The contraction of (9) to (10) is convenient in considering differences of total heat, but is not valid when absolute values of total heat are in question. It may be employed when the heat flux is expressed by (2), but not when it is expressed by (3). This argument is equally applicable to the case of a moist air-water system in which evaporation or condensation takes place, so long as there is no loss or gain of mass. Montgomery's conclusion that the heat flux through a constant pressure level depends not only on the air motion and the temperature and water content of the air, but also to some extent on the temperatures at which the water content was acquired, therefore appears not to be well-founded. The point is of some importance ; if Montgomery's contention were true it would not be possible to compute latent heat flux from observations of local wind and humidity, as has been done, for example, by Priestley (1949).

5. An examination of the arguments of Priestley and Swinbank

These authors attempt to demonstrate the relative importance of free and forced convection in the atmosphere by means of two types of argument which may be termed *physical* and *synoptic*. The physical argument concerns the fluctuations of temperature at a point in the atmosphere, the synoptic arguments involve considerations of the distribution of temperature in the vertical, both in general and on specific occasions, and of the magnitude and direction of the convective heat flux at the ground on a specific occasion. These arguments will be examined in turn in the following sub-sections.

(a) *The fluctuations of air temperature*

Priestley and Swinbank give examples of temperature fluctuations measured by an aspirated thermometer of lag-constant about 2 sec. They argue that the amplitudes and periods observed cannot be caused by forced vertical motion of air parcels in an air mass of reasonable lapse rate in which the horizontal temperature distribution was originally homogeneous. This cannot be denied, but it is equally true that free motion under buoyancy forces cannot set up the fluctuations observed by acting on an initially homogeneous stratification. The observations suggest a horizontal inhomogeneity of temperature imposed by external causes — this appears to be recognized by Priestley in a later communication (Priestley 1948) — and, once such inhomogeneity is in existence at one level, forced and free convection are potentially equally efficient agents in propagating it to other levels. Observations of temperature alone therefore provide no means of assessing the relative importance of the two forms of convection in the atmosphere.

(b) *The persistence of a minimum of potential temperature about a fixed pressure level*

On the Taylor-Brunt theory, since forced convection transfers heat only down the gradient of potential temperature, a minimum of potential temperature in the atmosphere cannot remain at a fixed pressure level without an increase of temperature. The amount of this increase may be estimated with reasonable accuracy in given circumstances. Priestley and Swinbank examined the records of routine aircraft temperature soundings made at Aldergrove, and claim to have discovered many instances in which a potential temperature minimum persisted at or near a given pressure level for several hours, with negligible change of temperature. They argue that the buoyancy forces provide the only mechanism available to explain these observations.

In considering the evidence for this assertion several general provisos must be kept in mind. In the first place potential temperature is not observed directly during the ascent ; the pilots have instructions to report temperature inversions and isothermal layers, but they have no means of detecting other changes of lapse rate. Secondly the temperatures obtained from routine aircraft soundings made for synoptic purposes are notoriously inadequate as guides, to the detailed temperature structure of the lower atmosphere, and temperatures are read and corrected to the nearest 1°F only (reference to pilots' original records confirms this in general, but there are occasional entries to the nearest 0·5°F ; the reporting code only allowed transmission of whole degrees). Thirdly the argument implies absence of advection and in this respect the choice of Aldergrove is singularly unfortunate since the proximity of Lough Neagh would be expected to give rise to frequent and persistent advective effects. Fourthly, the potential temperature appropriate to the problem in the presence of condensed water is the wet-bulb potential temperature. Fifthly, care must be exercised to eliminate the effects of transfer of heat to or from the layer considered by other methods such as the formation and dispersal of cloud layers, radiation from clouds, *etc.*

Little can be said on the general claim of Priestley and Swinbank to have covered numerous examples of a persistent stationary minimum of potential nperature in a single year's record, but a critical examination of the two situations lich they present as examples is possible. The relevant parts of these ascents, tted on the standard T-Φ-gram from the original Aldergrove records are shown Fig. 1. The crosses indicate the pressure and temperature at which Priestley l Swinbank claim a minimum of potential temperature. It will be seen that in eral cases these points are not on the curves now presented. The discrepancies presumably due to rounding off temperatures to the nearest degree for trans-ssion and to small differences in the computation of 1000 ft pressures. This istrates the unsuitability of routine aircraft soundings for the present purpose, rticularly if reference is not made to the pilot's original record. The following mments on individual occasions seem appropriate.

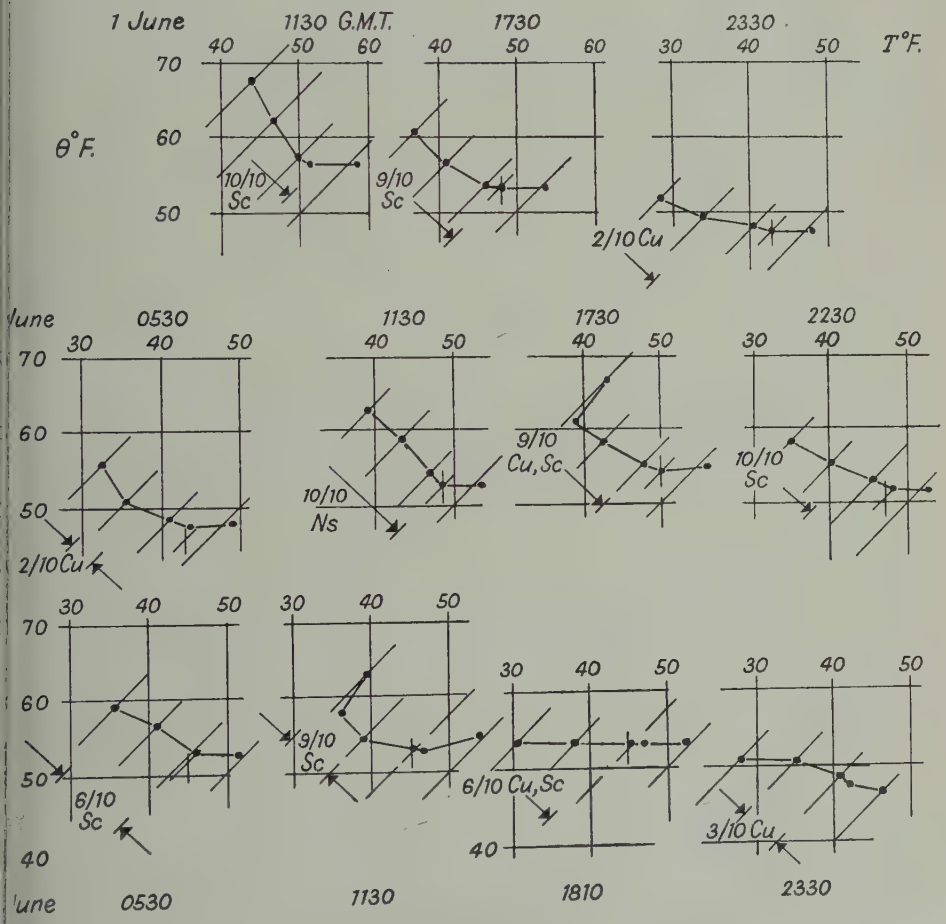

Figure 1. T-Φ-grams of Aldergrove ascents ; 1, 2, and 5 June 1944 ; surface to 850 mb.

Period, 1-2 *June* 1944

This is historically notorious as an occasion when a prolonged spell of fine hot anticyclonic weather was being replaced by a stormy westerly type with rain and gales. Advective effects would be expected to be marked, and are very obvious from the ascents. In addition a warm front with rain crossed the area on the 2nd.

1 *June* 1944, 1130 *G.M.T.*

This ascent was not used by Priestley and Swinbank, but is presented here to emphasize the advective effect.

1 *June* 1944, 1730 *G.M.T.*

There was 8/10 of cloud with base below the level of minimum Θ quoted by Priestley and Swinbank. Since wet-bulb potential temperature is the appropriate parameter in cloud the true level of heat accumulation may have been much higher. The minimum claimed is in any case the reported 1000 ft temperature. There were no temperatures measured between this and the surface, and the minimum of Θ can only be fixed as between surface and 1000 ft. No temperature singularity was reported by the pilot in this or any of the other cases studied.

1 *June* 1944, 2330 *G.M.T.*

Again the minimum Θ can only be fixed as between 1000 ft and the surface.

2 *June* 1944, 0530 *G.M.T.*

The minimum Θ can only be fixed as between 1000 ft and the surface. Priestley and Swinbank's point is in any case not on the curve as plotted from the original observations.

2 *June* 1944, 1130 *G.M.T.*

The minimum Θ coincides with the base of 10/10 Ns. and the level of heat accumulation may have been in cloud. In any case the presence of rain renders this example valueless.

2 *June* 1944, 1730 *G.M.T.*

The minimum Θ given by Priestley and Swinbank again coincides with the 1000 ft temperature. There was 9/10 cloud with base just above this level, and an increase of 0·5°F in the 1000 ft temperature (*i.e.* a quite likely error) would place the level of heat accumulation at the top of this cloud layer at 850 mb. The observations are therefore insufficient to locate this level more precisely than between the surface and 850 mb.

2 *June* 1944, 2230 *G.M.T.*

There is no obvious superadiabatic gradient, but there may have been one from the surface to a point below the 1000 ft level. The point given by Priestley and Swinbank for minimum Θ is not on the curve plotted from the original observations.

pd, 5 June 1944

This was a day with strong NW.'ly gradient (35 mi/hr) and frequent showers Northern Ireland, both on the coast and inland — hardly a day on which it pe claimed that examination of the synoptic chart shows that horizontal advection eat was not significant.

ne 1944, 0530 *G.M.T.*

There is no obvious superadiabatic gradient in the curve plotted from the nal observations : Priestley and Swinbank's minimum point is not on this e. There may, however, have been a point of minimum Θ between the surface 1000 ft. The lapse rate in the cloud (6/10, base reported only slightly above 1000 ft level) appears to be superadiabatic, so that a margin of error of 0·5°F ne 1000 ft temperature would place the level of accumulation of heat above mb. In view of the general weather situation this is not unlikely.

ne 1944, 1130 *G.M.T.*

The point of minimum Θ given by Priestley and Swinbank coincides with the mb observation. Strict interpretation of the record therefore puts the point eat accumulation between this level and the surface. The ascent may not ever be representative, since 9/10 Sc. cloud, base 900 mb, was reported but measured relative humidities in this layer were less than 80 per cent. The appears to have chosen a gap in the cloud layer, and the reported temperatures probably affected by this.

ne 1944, 1810 *G.M.T.*

Priestley and Swinbank's minimum Θ is again at the 950 mb observation, but in the limits of error of the temperature readings the lapse rate is adiabatic o 850 mb. The ascent appears to have been made in a gap in a 6/10 layer of and is therefore not fully representative.

ne 1944, 2330 *G.M.T.*

This ascent was not used by Priestley and Swinbank, but is included to show possibility of advective effects and the variation of cloud amount.

It is submitted that this re-examination of their examples suggests that Priestley Swinbank were insufficiently critical both in their choice of situation and their ment of the observations. It is also clear that routine upper air soundings by aft are inadequate to fix the level of minimum Θ, being too widely spaced eight and too imprecise in temperature observation. If however it could be blished that a minimum of potential temperature does persist for long periods given level, in the absence of advection, condensation or evaporation, or radiation cloud tops, the importance of the buoyancy effect would be demonstrated. ial observations to explore this possibility, *e.g.* by moored balloon, would efore be valuable.

(c) *The heat balance at the earth's surface in conditions of " anticyclonic gloom "*

When the lapse rate is sub-adiabatic down to the surface, the Taylor-Brunt theory demands that the convective flux of heat be downwards at the surface. In the absence of advective effects the other terms in the heat balance are the short-wave radiation, the long-wave radiation from the atmosphere and from the surface, the flux of heat to or from the soil, and the latent heat of evaporation, condensation, or freezing, at the surface. The convective heat flux may be estimated if all the other terms are known. Such measurements have been made by, *e.g.* Pasquill (1949) and Rider and Robinson (1950), and a number of examples in near-adiabatic conditions are shown in Table I. It will be seen that the convective flux appears as the small residual of a number of large and not very precisely measurable terms, and it is obvious that great care must be exercised in arguments concerning its magnitude even when careful simultaneous measurements of the other terms are available, and it is known that advection is absent.

TABLE I.—Some observations of the heat balance at a surface of short grass in near-adiabatic conditions

				Fluxes in units of 10^{-4} cal cm^{-2} sec^{-1}				
Observer	Date and time (G.M.T.)	State of sky	Lapse rate in lowest metre	Net short-wave radiative flux	Net long-wave radiative flux	Downward flux to soil	Latent heat flux	Convective flux by difference
R and R	11 Nov. 48 1400-1430	clear sky	Slight inversion	-18 ± 2	$+12\pm1$	$+3\pm1$	$+4{\cdot}5$*	$-1{\cdot}5$
R and R	16 Jun. 49 1605-1635	10/10 cloud	Moderate super-adiabatic lapse	-40 ± 4	$+19\pm3$	$+3{\cdot}5\pm1{\cdot}5$	$+12$*	$+5{\cdot}5$
Pasquill	10 Mar. 48 1500-1600	clear sky	Very slight inversion	-44	$+25.5$*	$+3$	$+16$	$-0{\cdot}5\pm7$
Pasquill	24 Mar. 48 1600-1700	clear sky	adiabatic	$-40{\cdot}5$	$+26$*	-7	$+15{\cdot}5$	$+6\pm7$

Fluxes directed away from the surface are considered positive.
*—Estimated value, see original paper for details.

The argument of Priestley and Swinbank concerns a well known weather type. Their statement of the data and their conclusions will be quoted.

" The winter situation known as anticyclonic gloom may persist for days and is characterized by extensive cloud of the inversion type at about 2000 ft, surface winds of 5 to 10 mi/hr, and a value of $(\partial T/\partial z + \Gamma)$ about 5×10^{-5}°C cm^{-1} at 50 ft. This latter figure is derived from detailed lower-atmosphere soundings at Cardington, and confirms that given by Johnson and Heywood (1938) (Table III, overcast days in December). The presence of low cloud enables us to arrive at a reasonably sound estimate of the quantities of heat involved in radiation. If the cloud temperature exceeds that at the ground, which is sometimes the case in these situations, and both are assumed to radiate as black bodies, then water vapour between cloud and ground approximately halves the black-body radiation interchange, and

inary temperatures the ground will gain heat at a rate of about 0·1 cal cm^{-2} hr^{-1} C temperature difference. This difference does not normally exceed 2°C. stimation of the latent heat quantities is made difficult by the lack of experi- l data ; but information received from Rothamsted Experimental Station (for we are grateful to Dr. H. L. Penman) indicates that in these situations ration may be negligible, or may even be negative, *i.e.* water is condensed e earth's surface.

he flux of heat through the 50 ft level may be estimated from the classical by taking K as 2×10^4 (Best 1935) Table XIII by inference ; Johnson and ood (1938) Tables XV and XXIII). The amount of heat flowing downwards days is then 36 cal cm^{-2}. The heat capacity of 50 ft of air can be neglected mparison with that of a few inches of soil, and measurements of changes of mperature show that nearly all the heat should go to warming the top 4 in. layer should then warm by 6°C. Records of earth temperature in the top of soil taken over the past two years at Dunstable and Cardington show that ch situations the change in sensible heat of the top layers of the ground in ays is almost negligible. Thus when consideration is given to the other heat ities involved, it appears that in fact the flux of energy due to eddying motion be a small fraction of the above 36 cal, and may even be directed upwards."

t will be seen that Priestley and Swinbank have no simultaneous measurements e quantities involved ; they offer a collection of semi-quantitative estimates in differing places at different times ; and for this reason alone their argument be considered inconclusive. It is, however, clear that their assumed data ot self-consistent. They take the cloud base to be at a temperature 2°C that of the ground surface ; with $\partial T/\partial z$ at 50 ft about -5×10^{-5}°C cm^{-1}. e weather situation considered there is no question of an inversion between and the cloud base, so that with cloud at 2000 ft the assumptions imply an ion of 5°C between 50 ft and the surface. Such an inversion is invariably panied by rapid warming of the ground, a fact which is recognized by Priestley winbank (see p. 559 of their paper), but which is quite inconsistent with their assumptions in the present argument. It is also unfortunate that they make lowance for heating by short-wave radiation, which even in the conditions dered is far from negligible.

t is therefore submitted that Priestley and Swinbank have not established proposition that in conditions of *anticyclonic gloom* in eastern England the ctive flux of heat is less than can reasonably be expected on the Taylor-Brunt y, and may even be directed upwards against the potential temperature gradient.

HE AVERAGE DISTRIBUTION OF TEMPERATURE WITH HEIGHT IN THE TROPOSPHERE

he most fundamental and universal of the arguments advanced by Priestley winbank concerns the normal distribution of temperature with height in the sphere. The main source of heat in the atmosphere lies at and near the e, the main sink in the upper air ; the normal direction of heat transport wards. But it is an observed fact that the potential temperature normally ases with height, in which case the Taylor-Brunt theory indicates a downward

convective transfer of heat. Priestley and Swinbank argue that there are many occasions on which convection is the only process capable of providing appreciable transfer of heat, that on some at least of these occasions potential temperature increases with height throughout, or almost throughout, the troposphere, and that free convection provides the only mechanism of upward heat transfer in these circumstances.

Now free convection while requiring horizontal inhomogeneities of temperature, will act, in a subadiabatic gradient, as a means of reducing these inhomogeneities, and continued transfer by free convection requires some other mechanism to perpetuate them. Furthermore the vertical distances through which such inhomogeneities can be propagated, if they are maintained in one level by an inhomogeneous source, is limited in a subadiabatic gradient. If the inhomogeneities are due to a forced convective flux of heat downwards, the opposing free convective flux cannot exceed its cause, the downward flux. If the inhomogeneities are located at the surface, owing to unequal heating, the vertical distance to which they can be propagated in a subadiabatic gradient is limited and therefore the vertical distance to which heat can be carried by free convection is limited. Free convection cannot therefore continue to convey heat upwards in a subadiabatic gradient, and its effects are limited and temporary, except insofar as it opposes, but does not exceed, the downward forced convective flux which is its cause.

The system proposed, with a heat source fed by radiation at the surface and a radiative heat sink in the upper atmosphere linked by an upward convective heat flux against the gradient of potential temperature, is not realistic. There are in fact no observational grounds for supposing it to exist. Priestley (1949) has demonstrated directly the importance of advective heat transfer in Southern England ; this advective flux is connected mainly with the processes which we have termed dynamical. Furthermore it is generally accepted that the development of the major weather systems is accompanied by transfer of heat from the main atmospheric heat source in low levels and low latitudes to the sink in high levels and high latitudes. The heat supply to the upper troposphere in regions of subadiabatic gradient is thus assured by large-scale processes ; there is no need to strike a heat balance within the restricted system we have used to define convection, and hence no imperative need to appeal to the buoyancy forces or to modify the Taylor-Brunt treatment of convection. The view here advanced is that, away from the regions of superadiabatic lapse, the vertical distribution of temperature is the resultant of advective, radiative, and convective processes, the effects of which are probably of the same order of magnitude. The only regions where convection is the controlling influence are the areas of violent penetrative convection (*e.g.* tropical regions and broad currents of arctic air moving equatorwards over the oceans) where both free and forced convection combine to convey heat upwards.

7. Conclusions

An attempt has been made to show that the arguments by which Priestley and Swinbank sought to demonstrate the importance of the buoyancy forces in atmospheric convection are not convincing. No alternative suggestions have been

e ; it seems that the relative importance of free and forced convection in any n situation, even with a superadiabatic lapse, can only be decided by measure- t of the work done by buoyancy forces and by turbulence shearing stresses ectively. It is however clear that if the buoyancy forces have appreciable t the sum of total heat and geopotential is not conserved in an adiabatic vertical lacement of an element of air, and that in these conditions the heat transfer depend on other properties of the temperature field in addition to the gradient otential temperature. It therefore does not seem possible to attribute importance ree convection and at the same time retain the Taylor-Brunt type of formula the vertical heat flux.

The bearing of this discussion on one problem of considerable practical impor- e may be mentioned. Priestley (1949, p. 34) and Pasquill (1949), have pointed that if the buoyancy forces have appreciable effect the use of the Bowen ratio ivide the total heat flux into sensible and latent-heat components is not justified. quill has produced experimental evidence of a failure of the Bowen ratio method land in extreme superadiabatic conditions, but this has not been confirmed Rider and Robinson (1950). It is very desirable that this question should be led since the potential value of the Bowen ratio method to the meteorologist, is sound, can hardly be overstated.

Acknowledgment

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Discussion

Messrs. C. H. B. Priestley and W. C. Swinbank (*communicated*) : " In the eme types of situation characterized by advective cooling and warming of the air ems that mechanical and convective turbulence respectively dominate the vertical flux. In the synoptic situations ranging between these two extremes it seems y that the one type of turbulence will gradually yield in importance to the other." s extract from the ' General Appreciation ' section of our paper contrasts with Robinson's summary that we have claimed ' universal importance ' for free

convection and his unqualified statement that we regard ' the significance of free convection as equal to or greater than forced convection in atmospheric processes.'

The circumstance that, five years ago, our viewpoint was new may have led us to omit the occasional warning clause or qualifying phrase, but in a critical review Robinson has hardly the same excuse. It is regrettable, therefore, that he should so misrepresent us at the outset and set an emphasis on our work far beyond that which we ourselves claimed.

Robinson chooses to represent our arguments as based mainly on synoptic grounds and concentrates his criticisms on this evidence. Considering the three points in turn :

(1) While admitting vulnerability in our treatment of the Aldergrove data, we cannot concede that 0.5°C errors in the reported temperatures altogether remove the difficulty. Advective effects would have had to be abnormally large to produce temperature changes comparable with those referred to in our paper.

(2) It is Robinson's assertion, not ours, that there can be no inversion below the cloud base in anticyclonic gloom ; and his suggestion that there should be an inversion of 5°C between 50 ft and the surface can only arise from a complete misunderstanding of the conditions we postulated. The temperature difference of 2°C between ground and cloud was suggested, not as an average, but in order to show that even a relatively generous allowance for long-wave radiation would not weigh heavily in the balance according to the classical viewpoint. Furthermore, he does not appear to realise that any contribution from *short*-wave radiation would only strengthen our case.

(3) The general explanation of the mean tropospheric temperature distribution has been unchallenged since Emden's day. Our concern (see p. 547 of our paper) was not with this but with the rarity of even local and temporary existence of near-adiabatic layers, and Robinson has ignored the time and depth factor which was the basis or our argument.

It should, moreover, be evident from the relative space allotted, in text and summary, that we regarded the synoptic applications as only incidental to the main, more fundamental, body of our work. They represented an attempt at quantitative application to synoptic problems of the facts known about surface turbulence. It was inherent in our purpose that we should employ 'semi-quantitative estimates made in different places at different times,' and it is only by the continuation and improvement of such attempts that surface turbulence will emerge from a somewhat detached, specialist field into an integral part of meteorology.

More recently Pasquill (1949), by an entirely different approach, has confirmed the theory in that K_H exceeds the other K's in lapse conditions. But Rider and Robinson have denied the validity of Pasquill's procedure and results also.

Our expression of the flux as

$$F = -\rho c_p K' \left(\frac{\partial T}{\partial z} + \Gamma\right)$$

may rightly be held to be artificial in that the physical significance of K′ is less immediate than that of the classical, purely kinematic, K. We have suggested else-

(*Q.J.*, **74**, p. 412) that a satisfactory expression for F must be in more complex than a simple function of the lapse rate. This served to emphasize what was tly shown in our original treatment, that K' must depend on the lapse rate, nematics of the mechanical turbulence, and on the degree of temperature ogeneity. We are unable to see in what way Robinson's remarks represent lvance from this point.

has been held against our treatment that we discussed the consequence of rature inhomogeneities without explaining how they came into being (though not argued against discussion of wind fluctuations). To us this appears an nd criticism, once the fluctuations have been shown to exist. However, the we did not discuss the causes of the observed fluctuations was that we re- one at least of them, *i.e.* that any natural surface is the source of inhomo- es in a multitiude of ways, as so obvious as to require no emphasis beyond on of '. . . . in the atmosphere' in the title. We would like, therefore, to asize now that we have considered from the outset a medium where external ces are continually operative in an irregular manner and throughout which, as t, there exist inhomogeneities not attributable to the motion itself. Moreover, urce of temperature differences is not restricted to the surface, for radiation rating in a medium which is not of uniform constitution ; the patchiness of is a manifestation and perpetuation of inhomogeneity. We produced and sed evidence of rapid fluctations at 3,700 ft, and see no reason to agree that opagation of inhomogeneities is limited to within any specified distance of the e.

would be, perhaps, unfair to the author to take him up on his opinion that t source fed by radiation at the surface and a radiative heat sink in the upper phere linked by an upward convective heat flux against the gradient of potential rature, is not realistic.' For this describes precisely one of the few atmos- mechanisms one had believed to be beyond dispute.

the time and space at our disposal it is not possible to dissect these and other in full. We did not originally, and do not now, claim the synoptic arguments more than tentative. Nevertheless our paper remains as a body of work soundly in theory with a variety of good *supporting* evidence. It is surely the direct terminations of the future, rather than the somewhat idle controversy of the t, which will finally measure its value.

DR. G. D. ROBINSON (*in reply, communicated*): I have been given the opportunity lying to the issues raised by Messrs. Priestley and Swinbank. In writing my I was of course less concerned with these authors' interpretation of their own than with its impact on other meteorologists, of which I have some knowledge. satisfied that the facts and opinions have now been clearly stated. There is vious cleavage of opinion as to what constitutes sound theory and good support- idence, and if this 'controversy' stimulates any reader to thought on the on I myself would not stigmatise it as 'somewhat idle.' I would, however, make two disclaimers. The parenthetical statement of Messrs. Priestley winbank concerning wind fluctuations bears no relation to anything which written, or even thought, and I am very sorry indeed that they should con- me ignorant of the direction of the net flux of short-wave radiation.

551.511 : 532.59

On the stability of stably stratified shearing layers

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SUMMARY

The equation governing the behaviour of two-dimensional waves in a horizontal stably stratified shearing air current is set up. It is shown that it may be obtained by considering the limit as the number of a set of superposed statically-neutral layers, each of constant stream velocity, tends to infinity, but that the properties may differ considerably if the number of layers is few.

If stable waves occur then at no level do they travel with the same velocity as the air. If unstable waves occur the surfaces containing the wave crests may be so inclined to the vertical as to limit the growth of the waves. Some remarks are made concerning the difficulties of discussion of unstable waves in the general case.

1. INTRODUCTION

It is well known that horizontal surfaces of discontinuity of velocity between homogeneous fluids are unstable for disturbances of wavelengths in a certain range. This fact has been used, first by Helmholtz and later by many other authors to explain regular cloud bands on the assumption that they occur at velocity discontinuities, the cloud being concentrated in the wave crests. It seems more probable that sharp discontinuities are exceptional and layers with a larger velocity gradient than above and below are more common. It is therefore worth while to know if shearing layers do in fact behave like discontinuities. Sekera (1948) has considered the case of uniform shear with stable stratification in two or three superposed layers, and has shown that stable waves travelling with the same speed as the air at some level can only occur if in a layer around that level the air is not statically stable. This result is confirmed by a different method in section 3(b)*. The immediate consequence is that a single discontinuity behaves with regard to stable waves in a completely different manner from a layer of gradual change of velocity ; for stable waves can travel in the former case with a velocity intermediate between the two layers.

The general case of a continuous velocity change with stable stratification throughout is investigated for two-dimensional motion, and it is shown how difficult it is to investigate the possibility of unstable waves. There are certainly no sure grounds for assuming that waves which are unstable for the case of a single discontinuity can even occur in the more general case, and it has been thought worth while to point this out without waiting for what must be a very long numerical investigation of some particular examples to be made.

Before proceeding it seems appropriate to make some remarks on the general ideas behind the study of unstable gravity waves. If one can show that under certain circumstances waves of an observed length are unstable, then those cir-

* A fuller discussion of what the motion could be like when stable waves do have the same velocity as the air is reserved for a later paper.

stances might be offered as an explanation of observed wavelike phenomena. in all the cases investigated theoretically so far, a whole range of wavelengths been found unstable while no adequate reason is given why waves of one length should appear. No description has been given as to how a state of finite mic instability could be reached ; one would expect the system to break down e course of its being set up as soon as an infinitesimal range of wavelengths me unstable. Nor has any criterion been offered as to how unstable a system be before it breaks down. Indeed it seems reasonable to expect that the s which first become unstable as the unstable state is set up would be the to appear, but this aspect has never been investigated. Furthermore, although certain systems waves which are unstable can be found, it has not generally proved that by such wave motions the system breaks down most readily. ugh in some simple cases proof of this could be given, more complicated systems t be capable of a different kind of instability which occurs more easily than unstable waves.

The concept of dynamic instability is often very valuable. It can throw light he processes leading to changes in energy distribution, and it can tell us of mstances when small changes in initial conditions can lead to large differences ubsequent development. A classical example of its success is in explaining lapping of flags. The flag suddenly introduces a surface at which the tangential city may be discontinuous, but it is difficult to imagine how such an unstable could be so rapidly produced in the atmosphere, except possibly by the con- ation of water vapour.

Alternatively stable waves may be used to describe the appearance of billow ds, but here again the explanation is incomplete without a mechanism for ng up the waves and for picking out one particular wavelength from all those ible.

The idea of Haurwitz (1947) and Sekera (1948) to associate Helmholtz waves convection cells is reserved for later discussion ; in the present paper we ribe some of the ways in which static stability and shear affect the wave properties n air current. It will be evident that no precise statements can be made about properties or even the existence of unstable waves in the general case.

2. Waves in a fluid of several homogeneous layers

This problem was studied by Taylor (1931) and Goldstein (1931), but apart n a brief summary of Taylor's results by Brunt (1939) their papers seem to have ed unnoticed in meteorological literature. The mathematical difficulties and ncompleteness of their conclusions may be responsible for this, but both authors ark unequivocally that the properties of a fluid with a continuously varying city and density cannot be deduced by considering many layers in each of h they are constant. The instances in which a continuous variation was ssed were particular and it seemed desirable to discover whether any general lusions applicable to the atmosphere could be drawn. It is here demonstrated if the number of discrete layers is increased indefinitely the case of a continuous tion is arrived at in the limit.

In each layer the horizontal velocity U and the density ρ are uniform and the motion is assumed to occur in vertical planes. The vertical displacement of the base of a layer is denoted by ζ and its height by h, ϕ is the velocity potential and p the pressure. In the nth layer, numbered upwards, all quantities are denoted by suffix n. (See Fig. 1).

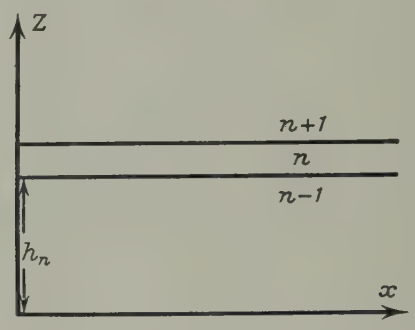

Figure 1.

There is now supposed to be a small wave disturbance which is stationary relative to the coordinate system, and so writing

$$\zeta_n = B_n \cos kx \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

then (see, for instance, Lamb, *Hydrodynamics*, Art. 228)

$$\phi_n = -U_n x + U_n (C_n e^{kz} + D_n e^{-kz}) \sin kx \quad . \quad . \quad . \quad (2)$$

where B_n, C_n, and D_n are constants for the nth layer. The pressure is given by

$$\frac{p_n}{\rho_n} = -\tfrac{1}{2}\left\{\left(\frac{\partial \phi_n}{\partial x}\right)^2 + \left(\frac{\partial \phi_n}{\partial z}\right)^2\right\} - gz + \text{const.}$$

$$= -gz + U_n{}^2 k \cos kx (C_n e^{kz} + D_n e^{-kz}) + \text{2nd order terms} + \text{const.} \quad . \quad (3)$$

Putting the pressure at the base of the $(n + 1)$th layer equal to that at the top of the nth and retaining only first order terms we get

$$\rho_n \{ U_n{}^2 k (C_n e^{kh_{n+1}} + D_n e^{-kh_{n+1}}) - g B_{n+1} \}$$

$$= \rho_{n+1} \{ U_{n+1}{}^2 k C_{n+1} e^{kh_{n+1}} + D_{n+1} e^{-kh_{n+1}}) - g B_{n+1} \} \quad . \quad . \quad (4)$$

The vertical velocity in the nth layer is

$$-\frac{\partial \phi_n}{\partial z} = -U_n k \sin kx (C_n e^{kz} - D_n e^{-kz}) \quad . \quad . \quad . \quad . \quad (5)$$

while at $z = h_n$ it is also equal to

$$U_n \frac{\partial \zeta_n}{\partial x} = -U_n k B_n \sin kx \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

uating these $C_n e^{kh_n} - D_n e^{-kh_n} = B_n$ . . . . . (7)

l similarly at $z = h_{n+1}$

$$C_n e^{kh_{n+1}} - D_n e^{-kh_{n+1}} = B_{n+1} \quad . \quad . \quad . \quad . \quad (8)$$

If ρ_n, U_n, B_0 and some other boundary condition (such as the pressure at a e surface) are given, then equations (4), (7), and (8) suffice to determine all the fficients C_n and D_n. In each layer the motion is irrotational, but each interface vortex sheet because the disturbance of horizontal velocity is not continuous. ve now let the layers increase in number indefinitely then we shall approach a tinuous distribution of velocity and density. The motion becomes rotational the limit so that there is no longer a velocity potential. ϕ has however been d merely as a convenience to obtain (7) and (8) which do not contain it. Sup- ing that all the layers are of equal thickness ϵ, $= h_{n+1} - h_n$, then (7) and (8) yield

$$-C_n = (B_n e^{-kh_{n+1}} - B_{n+1} e^{-kh_n})/2 \sinh k\epsilon$$
$$-D_n = (B_n e^{kh_{n+1}} - B_{n+1} e^{kh_n})/2 \sinh k\epsilon$$

bstituting in (4) we find that

$$\rho_n U_n^2 k(B_n - B_{n+1} \cosh k\epsilon) - \rho_{n+1} U_{n+1}^2 k(B_{n+1} \cosh k\epsilon - B_{n+2})$$
$$+ (\rho_{n+1} - \rho_n) g B_{n+1} \sinh k\epsilon = 0 \quad . \quad . \quad . \quad . \quad (9)$$

w letting $\epsilon \to 0$ and dropping the suffix n so that ρ, U, and B become functions z, and putting

$$\begin{aligned} \rho_{n+1} &= \rho + d\rho \\ U_{n+1}^2 &= U^2 + d(U^2) \\ B_{n+1} &= B + dB \\ B_{n+2} &= B_{n+1} + dB_{n+1} = B + 2dB + d^2B \\ \text{Sinh } k\epsilon &= k\epsilon + 0(\epsilon^3) \\ \text{Cosh } k\epsilon &= 1 + \tfrac{1}{2} k^2 \epsilon^2 + 0(\epsilon^4) \\ \epsilon &= dz \end{aligned} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

n (9) assumes the differential form

$$B'' + \{(U^2)'/U^2 + \rho'/\rho\} B' - \{g\rho'/U^2\rho + k^2\} B = 0 \quad . \quad . \quad (11)$$

ere a prime is used to denote $\partial/\partial z$. B is the amplitude of the disturbance at ght z and this equation determines how B varies with height, ρ and U being own functions of z. Writing $B \propto w/U$ the equation for the vertical velocity is

$$w'' + w'\rho'/\rho - (g\rho'/U^2\rho + U''/U + \rho' U'/\rho U + k^2) w = 0 \quad . \quad . \quad (12)$$

ich is equivalent to that given by Taylor (1931), so that as the number of layers ncreased indefinitely the correct equation is arrived at in the limit. However ors may arise in two ways : (a) it is generally assumed that no essential simpli- tion is introduced if the shear is taken as uniform (the only case of which dis- sions have been published) while it has been shown (Scorer 1949) that the term $''/U$ can be important ; and (b) Taylor's and Goldstein's warning must be ded because it emerges in section 3(b) below that the possible range of velocities stable waves is reduced if the velocity profile is continuous, so that results may significantly different for a profile of a few discrete steps.

3. Waves in a stably stratified shearing current

(a) *The meteorological equation*

The meteorological form of Eq. (12) is now derived again. It has already been given in essence by Taylor, for an incompressible medium, and by Scorer for the case of steady motion. The present derivation is simpler for the immediate purpose. The discussion is restricted to waves whose velocity and wavelength is independent of height and the disturbance is supposed to contain the factor $\exp i(kx + \sigma t)$. U is the undisturbed velocity of the stream and $V, = U + \sigma/k$, is the velocity of the air relative to the wave form. A prime is used to denote $\partial/\partial z$; ρ_0 and p_0 are the undisturbed values, ρ and p are the disturbance of density and pressure. Products of small quantities are neglected throughout.

Although we assume a variation in velocity with height and so imply a thermal wind, no horizontal variation of density is supposed. But the wavelengths considered are such that the earth's rotation can be ignored in its effect on the disturbance; for normal wind speeds this restricts discussion to wavelengths less than about 30 km, and in this distance the density only varies horizontally by a very small fraction and may be considered constant.

We write

$$D/Dt \equiv \partial/\partial t + (U+u)\,\partial/\partial x + w\partial/\partial z$$

The equation of continuity is

$$D(\rho_0+\rho)/Dt + (\rho_0+\rho)\,\mathrm{div}\,(U+u, w) = 0$$

giving
$$-ikV\rho/\rho_0 = iku + (\partial/\partial z + \rho_0'/\rho_0)\,w \quad . \quad . \quad . \quad . \quad (13)$$

The horizontal component of the equation of motion is

$$D(U+u)/Dt = -\partial(p_0+p)/(\rho_0+\rho)\,\partial x$$

giving
$$-ikp/\rho_0 = ikVu + U'w \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

and on differentiation

$$p'/\rho_0 = -(\rho_0'V/\rho_0 + V' + V\partial/\partial z)\,u - (\rho_0'U'/\rho_0 + U'' + U'\partial/\partial z)\,w/ik\,. \quad (15)$$

The vertical component of the equation of motion is

$$Dw/Dt = -\partial(p_0+p_0)/\rho_0+\rho_0)\,\partial z - g$$

therefore $\rho_0 ikVw = -p_0' - p' - g(\rho_0+\rho)$

The equation for the undisturbed state is

$$0 = -p_0' - g\rho_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

so by subtraction we have

$$ikVw + p'/\rho_0 + g\rho/\rho_0 = 0$$

Substituting for p' and ρ from (15) and (13) we get

$$(k^2 + g\rho_0'/V^2\rho_0 + U''/V + gV^{-2}\partial/\partial z)\,w + (g/V^2 + \partial/\partial z)\,iku = 0 \quad . \quad (17)$$

ıe assumption that $\rho_0'U/\rho_0$, $U' << g/V$, and that V is smaller than or of ame order as U.

The equation for adiabatic motion is

$$D(p_0+p)/Dt = \gamma p_0 D(\rho_0+\rho)/\rho_0 Dt$$

$$ikV(p/p_0-\gamma\rho/\rho_0)+(p_0'/p_0-\gamma\rho_0'/\rho_0)w=0 \quad . \quad . \quad . \quad (18)$$

ming that $p_0 = R\rho_0 T_0$ and writing $c^2 = \gamma RT_0$, using (16) we have

$$p_0'/p_0 = -\gamma g/c^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

tituting now in (18) for p, ρ, and p_0'/p_0 from (14), (13), and (19) we have

$$iku = (g/c^2-\partial/\partial z)w \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

ıe additional assumption that $V << c$. If u is eliminated from (17) and (20) quation found for the vertical velocity is

$$w''-w'g/c^2+(g\beta/V^2-U''/V-k^2)w=0 \quad . \quad . \quad . \quad (21)$$

e $\beta = \theta_0'/\theta_0 = -\rho_0'/\rho_0 - g/c^2$, and θ_0 is the undisturbed potential tem- ture. The coefficient g/c^2 changes slowly by less than 20 per cent. throughout whole troposphere. Assuming it to be constant, the substitution $W\exp(gz/2c^2)$ leads to the equation

$$W''+(g\beta/V^2-U''/V-k^2+g^2/4c^4)W=0$$

ractical cases $g\beta/V^2 >> g^2/c^4$. Also for wavelengths of 2 to 3 km or less $> g/c^2$, so that the last term in the coefficient of W can be ignored. At the time, for layers of depth less than about 5 km $\exp(gz/2c^2) \simeq 1$ and $w \simeq W$ hat

$$w''-(-g\beta/V^2+U''/V+k^2)w=0 \quad . \quad . \quad . \quad . \quad (22)$$

Comparing this with Eq. (12) we see that in the meteorological case the nd term and the last can be ignored and the stability is represented by lapse- of potential temperature instead of density.

(b) *Stable waves*

It is now proved that if stable waves exist then they must be moving relative ıe air at all levels. For if we suppose that $V = 0$ at some level, say $z = 0$, near $z = 0$ we may assume that $V = \alpha z^q$ where $q > 0$. Eq. (22) then mes

$$w''+\{g\beta\alpha^{-2}z^{-2q}-q(q-1)z^{-2}-k^2\}w=0 \quad . \quad . \quad . \quad (23)$$

When $q > 1$, $z = 0$ is an irregular singularity of this equation, which means as $z = 0$ is approached w increases indefinitely in amplitude with oscillations er increasing frequency, and the nodal surfaces, at which $w = 0$, are crowded more closely together ; or, in mathematical terminology, $z = 0$ is a limit t of zeros of w. Nature does not permit the existence of such motion.

When $q \leqslant 1$, putting $w = \chi z^{\frac{1}{2}}$, $\zeta = ikz$, Eq. (23) becomes, near $z = 0$,

$$\frac{d^2\chi}{d\zeta^2} + \frac{1}{\zeta}\frac{d\chi}{d\zeta} + \left\{(A^2 - \tfrac{1}{4})\frac{1}{\zeta^2} + 1\right\}\chi = 0 \qquad . \quad . \quad . \quad . \quad (24)$$

which is Bessel's equation of order ν, where

$$\nu^2 = \tfrac{1}{4} - A^2$$

and $A^2 = g\beta/\alpha^2$ when $q = 1$,

$= -q(q-1)$ when $q < 1$.

We also note that for $0 < q \leqslant 1$, $\nu \leqslant \frac{1}{2}$,

$$\nu + \tfrac{1}{2} - q \leqslant 0 \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

equality holding only when $q = 1$, $\nu = \frac{1}{2}$, corresponding to $\beta = 0$; or when $q = \frac{1}{2}$, $\nu = 0$.

When ν is real, i.e. $q < 1$, or $q = 1$ and $\alpha^2 > 4g\beta$, the solution which tends most rapidly to zero as $z \to 0$ is $\chi = J_\nu(ikz)$, which near $z = 0$ is proportional to z^ν. The vertical displacement of the air is proportional to w/V, *i.e.* to $z^{\nu+\frac{1}{2}-q}$ which tends to infinity as $z \to 0$, in view of (25). This again is an impossible situation, except possibly when $q = 1$, $\beta = 0$ (the case discussed by Taylor), or when $q = \frac{1}{2}$; but even then the other solution of (24) diverges more rapidly than $J_\nu(ikz)$, and to exclude this is to impose an internal boundary condition which cannot be admitted, unless possibly it is a condition determing k. This is scarcely a practical criterion for deciding the wavelength that could appear in nature because it can only be applied for these very special velocity profiles.

When ν is imaginary, *i.e.* $q = 1$ and $\alpha^2 < 4g\beta$, the general solution is, putting $\nu = i\mu$,

$$\chi = aJ_{-i\mu}(ikz) + bJ_{i\mu}(ikz)$$

where a and b are constants. For small z this solution is proportional to

$$\mathrm{Cos}(\mu \log kz + \epsilon)$$

where ϵ is a constant depending on a and b. Thus again we have $z = 0$ as a limit point of zeros of w, and although w does not increase indefinitely in amplitude as $z \to 0$ the vertical displacement does.

We find therefore that in the case of stable waves there is no level at which $V = 0$. The result has the same physical significance as Sekera's, that waves can only occur if the air is statically unstable in a layer surrounding the level at which the waves are stationary relative to the air. It is well known that stable waves can and do occur, a common case being waves in the lee of mountains, but they are moving relative to the air at all levels. The possibility of motion which is like stable waves at all levels except near where $V = 0$ is outside the scope of the present paper.

(c) *Unstable waves*

If unstable waves exist then σ is complex and V is complex. Suppose that $V = l + im$, then the coefficient of w in (22) is

$$-k^2+\frac{g\beta(l^2-m^2)}{(l^2+m^2)^2}-\frac{U''l}{l^2+m^2}-i\left\{\frac{2g\beta lm}{(l^2+m^2)^2}-\frac{U''m}{l^2+m^2}\right\}$$

If β and U'' are not both zero, then the imaginary part does not vanish, which ns that $\partial/\partial z$ is also a complex quantity and so the phase of the wave form varies height. (This fact was pointed out to the author, in the course of a discussion, Dr. E. T. Eady). If the surfaces containing the wave crests are very much ned to the vertical a serious limitation may be placed on the growth of the e since large velocities may soon be achieved where a trough lies just over a , and the perturbation equations may very easily become invalid in the develop- t.

Now we have seen that stable waves must be moving relative to all the air. e assume that V is nowhere very small compared with its value at any other and that the wavelength is small so that

$$k^2 >> g\beta/V^2 - U''/V$$

$$w'' = k^2 w$$

if the waves die away at great height the only permissible solution is e^{-kz}, which means that if $w = 0$ at the ground then $w = 0$ everywhere, and e can be no waves. If however the coefficients involving V, and therefore z, important, then there may exist nodal surfaces (*i.e.* $w = 0$ at the ground) and condition $w \to 0$ as $z \to \infty$ may also be imposed. If the wavelength is given the position of the nodal surface will determine the phase velocity. As the elength becomes shorter it becomes progressively more difficult for the waves ccur as stable waves, but it may be that the waves of short length are unstable. his is so then at some level V must become so small that other terms in the ficient of w are comparable with k^2. Thus l must become small, and probably shes, at some level (*i.e.* the waves move with the wind at some level), and a is placed on the order of magnitude of m, for now $g\beta/m^2$ is not small compared k^2, *i.e.* mk is of the order of $(g\beta)^{\frac{1}{2}}$ or less. The waves grow like the factor $\simeq \exp(tg^{\frac{1}{2}}\beta^{\frac{1}{2}})$, which depends upon the stability and not on the wavelength, then only on the stability at the level where the waves are moving with the which in turn is determined by the top and bottom boundary conditions. For a rated adiabatic lapse rate $(g\beta)^{\frac{1}{2}} \simeq 10^{-2}$ sec^{-1}, so that an unstable wave form will ble its size in a time of the order of a minute, or more slowly.

If the rate of growth is not very dependent on the wavelength there is no on why a disturbance should not retain its initial form throughout growth. It nt be argued that an initial small disturbance can be regarded as composed of vavelengths, and the component wavelength which grows fastest will sooner ater predominate, so that a single disturbance develops into a train of waves ne most unstable length. In this problem the rates of growth are probably not rent enough for one wavelength to predominate before the perturbation mptions become invalid.

If there is a layer of neutral stability, say a cloud layer, then these considerations y unless the waves move with the wind in this layer, and then k^2 is of the same r as U''/m, and km is of the order U''/k, and so the longer of the waves in

the unstable range will probably grow quickest. It is notable that U'' may be important in determining the growth rate. Sekera's argument whereby he ignores U'' is valid only for stable waves, which are what he discusses.

These arguments are admittedly tentative, but they serve to illustrate that the shear and stability can play a predominant part in determining whether waves of certain lengths are initially unstable, and if so to what extent. A direct attempt to discover whether, with a given wind profile and stability, waves of a certain length are unstable would be purely numerical, since there is no formal solution to Eq. (22). It would be necessary to start at one boundary and perform a numerical integration to the other and see if the required second boundary conditions were satisfied. Trial values of the two constants, n, $= l - U$, and m would have to be made until this second set of conditions was satisfied. This second set would take the form of a complex equation and there would be two lines in the n, m plane corresponding to the real and imaginary parts of the equation being satisfied ; the point at which they crossed would give the required values of n and m. A more profitable approach to begin with would be to put $m = 0$ and find the limits of k within which a value of n can be found to satisfy both boundary conditions. This would mean that the equation to be integrated was real, and much easier to integrate than a complex one ; but it would only define the range of possible stable waves. Waves of other lengths would not necessarily be unstable since they might not be able to occur at all.

Discontinuities of temperature may often occur but are unimportant, while discontinuities of velocity are certainly much more rare and even if they do occur their existence would be difficult to confirm. Since discontinuities must be regarded as the exception rather than the rule one must be wary of taking a phenomenon, such as billow cloud formations, as evidence of an unstable wave motion which has only been investigated in a few very special cases.

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The electric charges and masses of single raindrops

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Summary

Raindrops were admitted by a cone-shaped opening into an insulated vessel connected to a valve amplifier for measuring the drop charge Q. Drop radius a was found by the absorbent paper method. The field at the ground and the point discharge current I were also measured. Most often, but not always, the signs of the drop charge and field were opposite. During point discharge, 173 drop charges and masses were measured, and statistical analysis showed a definite correlation between Q and $I-$, a probable correlation between $Q/-I$ and a and a possible correlation between a and $-Q$. By grouping drops according to radius, and taking averages within the groups, the approximate relationship $Q/-I \propto a$ was obtained. In one period when the radius was nearly constant, the observations gave the connection $Q \propto -I$ without taking averages. The observed relation between Q and I corresponds to the results of Simpson (1949) for the charge on a volume of rain. The observed charges were mostly much higher than predicted on Wilson's ion-capture theory, unless the field increases considerably with height in the atmosphere..

1. Introduction

There appears to be an intimate connection between the separation of electric charge in clouds and the particles of ice or water which fall within the clouds and ultimately drop out as precipitation. It is therefore likely that a study of the charges on precipitation will yield information about the processes of charge separation in clouds. Moreover, measurements of the charges on single drops may provide information which would be masked in the average results obtained by measuring rain currents comprising many drops. In any case, the shorter the total time occupied in a measurement, the more nearly do meteorological conditions approach to a steady state.

Previous measurements of the charges on single drops have been made by Gschwend (1922), Banerji and Lele (1932), Chalmers and Pasquill (1938) and Gunn (1947, 49, 50). Of these, Chalmers and Pasquill measured only the charges, while Banerji and Lele were not concerned with small charges and have only given a brief account of their results, leaving a doubt as to whether they measured the size of each drop ; Gunn (1947, 1950) measured charges, but not masses, of drops inside clouds, and also (1949) the charges and masses of drops at the ground during thunderstorms but not the simultaneous fields. Gschwend measured charge, mass and field, but his observations occupied at least 2 min per drop and often longer, while Chalmers and Pasquill observed drops at much shorter intervals.

The object of the present work was to combine the advantages of the earlier methods of Gschwend, and Chalmers and Pasquill, by measuring in a short period of time both mass and charge of a number of drops as well as the simultaneous field and the point-discharge current, if any.

2. Experimental procedure

The sizes of individual raindrops were measured by the absorbent paper method described by Wiesner (1895) and developed by Defant (1905), after other methods had been tried unsuccessfully. At the same time the electric charge carried by each drop was amplified electronically and recorded on a photographic drum. The vertical electric field and the point-discharge current were also recorded.

The raindrop receiving vessel (see Fig. 1) consisted of two parallel metal plates AA, BB, 16 cm square, 4 mm apart and connected electrically. A circular

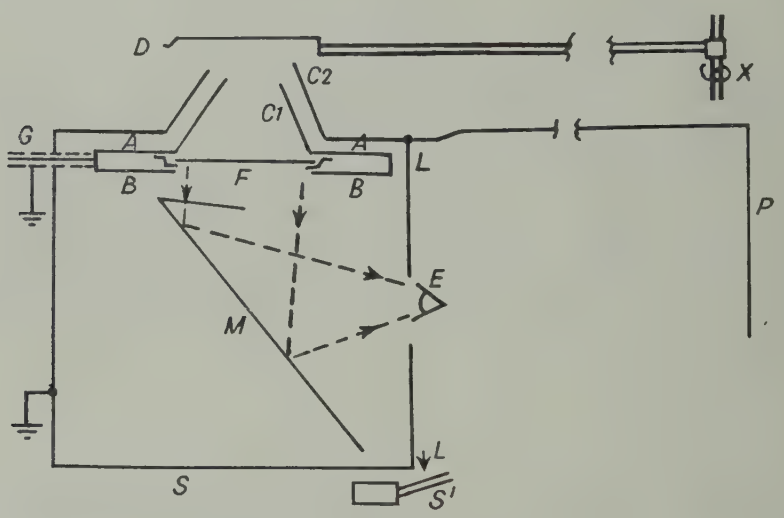

Figure 1. Raindrop receiver

hole, diameter 11 cm, was cut in the centre of each plate. An open copper cone C_1 was soldered to AA, the aperture diameter being 3·8 cm. A movable circular tin tray F just fitted easily between the plates AA, BB. It had a circular hole 10·5 cm in diameter cut out centrally, and over this hole was clipped a filter paper 11 cm in diameter, previously dusted with a red dye (Rhodamine B·S) soluble in water. The tray F, the paper, the plates AA, BB and the cone C_1, together formed the electrical receiver and were insulated from the earthed screening box S by polystyrene bushes. When the earthed cover D was moved clear by depressing a pedal, a rain drop entering the receiver through the outer cone C_2 would fall on to the filter paper at F. The red stain resulting was immediately apparent to an observer at E, looking into the inclined plane mirror M through a hole in the sliding cover LL. The cover D was then returned by releasing the pedal, and the used tray with the filter paper was quickly replaced by another and the process repeated, the charge of the drop being automatically recorded on the photographic drum.

The spring-loaded switch S′ controlled a shutter which interrupted the beam of light for the recording drum. The exposure of this beam was made to coincide with the exposure to rain of each filter paper. The photographic record therefore consisted of a sequence of traces whose lengths were proportional to the times of

xposure of the corresponding filter papers. The operator counted seconds from ie beginning of each exposure, and, on withdrawing the tray F, wrote against the indrop stain the number of the second at which the stain had appeared. Later, fter developing the photographic record, and knowing the speed of the drum, he ould associate the raindrop stain with the part of the record belonging to the same econd of time. With practice it became possible to observe two or three stains n one exposure. For most rains, a cone with an aperture diameter of 1·8 cm was tted over C_2 ; this admitted drops at a rate convenient for the observations. The highest rate of measurement was 45 drops in 14 min. The apparatus was ounted firmly on the flat roof of a hut 7 ft high ; a shelter P (see Fig. 1) protected ie observer and his pile of prepared trays from the rain.

A calibration curve was made relating drop radius and stain diameter for the lter paper used (English Postlip Milled, Quality No. 633, white). The larger drops ere obtained with drawn-out glass capillary tubing, the smaller by means of a icro-burette as described by Lane (1947). For the larger drops the radius was ound by counting 10 or 20 drops into a bottle and weighing ; the smaller drops ere caught in oil and the radius measured directly with a travelling microscope, s described by Nolan and Enright (1922). The papers were prepared by dusting n the dry dye powder and were stored in a dry box. To measure the stain size a uarter-plate lantern-slide was prepared by photographing a sheet of graph paper uled in millimetre squares. This served as a reference grid with an error of less han one per cent. A stain was never quite circular, so the mean diameter was ound by measuring the greatest and least diameters with the grid.

The amplifier (see Fig. 2) was based on that described by Chalmers and Pasquill 1937), but with developments in valves only one stage of amplification was required.

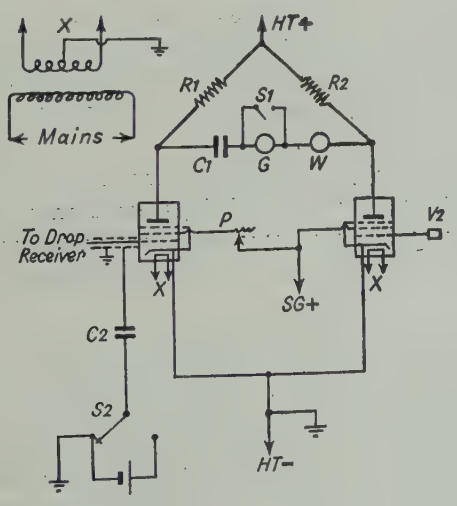

Figure 2. Amplifier

R_1, R_2 . . . . 2000Ω.	P . . . . 20,000Ω.
HT . . . . . 120V.	SG . . . . . 108V.
C_1 . . . . . 20μF.	X . . . . ~4V.

Two SP 41 radio-frequency pentodes were arranged in a bridge circuit. The anode potentials were made equal by varying the resistance P in the screen grid circuit of one valve (the condenser C_1 being temporarily short-circuited). The operating valve V_1 with grid floating, was connected to the raindrop receiver by a screened cable ; the grid of the balancing valve V_2 was left disconnected. W was an insensitive table galvanometer for coarse adjustments and G a dead-beat mirror galvanometer used ballistically. When G was not in use it was protected by closing the switch S_1. If a charge q arrives at the grid of V, it is amplified to give a charge C_1R_1Mq/K, where M is the mutual conductance and K the total grid capacity of V_1. The theory is similar to that given by Chalmers (1949). The small fixed air condenser C_2, mounted rigidly in the same screening can beside V_1 was used for day-to-day calibration. A known potential was applied by a switch S_2 and the deflection for the corresponding charge observed. C_2 was standardized by using in the same manner a Sullivan precision air condenser giving a 10·0 $\mu\mu$F increment. This standardization was checked by using equally charged water drops delivered steadily by a screened water dropper. First the deflection with the amplifier was observed and then the charge of three of four thousand drops together was measured with an ordinary ballistic galvanometer, correcting for leakage during counting ; this check agreed to within 5 per cent. with the value of 2·41 cm previously found for C_2. An experiment in which the distance of fall of the drops was varied showed that the speed of the drops on entering the receiver had no effect on the amplification. The limit of charge measurement, due to fluctuations of the zero, was $0{\cdot}1 \times 10^{-3}$ ESU.

The vertical electric field was found from the bound charge recorded when the cover D was moved to cover or uncover the cones C_1, C_2 ; C_1 is not completely shielded from the field, but field changes (due to lightning) would only vitiate the results if they happened to occur simultaneously with a drop observed visually. The pedal controlling D was operated at definite times during an observation, so the deflections for the bound charge could be distinguished easily on the record. Calibration of field measurements was made with a stretched wire 1 m high carrying a glowing fuse and using an electroscope.

The point-discharge current was that flowing into a platinum-tipped vertical rod, similar to that used by Wormell (1930), erected on the corner of a building 30 ft high and connected to earth through a galvanometer. The galvanometer deflections were recorded on the same drum as the raindrop charges. A double beam of light was used, giving a zero trace along each edge of the photographic paper ; the whole width of the paper was thus available for recording deflections of either sign, and the zero line did not interfere with the drop-charge records.

During point discharge, the field and the point-discharge currents were simultaneously measured and showed a general relation similar to that of Whipple and Scrase (1936) ; this is to be published separately and will not be considered further here. At the time of arrival of a drop, the point-discharge current is directly measured, but the field can only be obtained by interpolation between the neighbouring times of movement of D. In discussion of the results, it is the point-discharge current, when present, which is used rather than the field, except in Table IV.

3. Observational results

(a) *Signs of drop charge and field*

Between December 1947 and October 1948 a total of 54 periods of rain yielded alues of both electric charge and drop radius for 1,128 single drops. There were ther drops for which the charge only was measured, and in many periods, usually f quiet continuous rain, charges were too small to be recorded.

The numbers of drop charges of either sign are given according to the kind f rain and the sign of the field in Table I. Rain is classified as follows :

Continuous rain — when the rate of rainfall remains practically constant for at least one hour.

Thunderstorms — when thunder is heard.

Showers — all other kinds of rain.

TABLE I. Number of drops in relation to sign of field and charge

	Field positive			Field negative		
Sign of charge	+	—	Zero	+	—	Zero
Continuous rain	22	127	65	230	134	348
Thunderstorms	7	38	2	28	19	4
Showers	35	82	57	124	69	85
Totals	64	247	124	382	222	437

Considering the totals statistically, if the zero charges are neglected χ^2 is 161, nd if the zeros are divided equally among positive and negative χ^2 is 104. This ows (Fisher 1932) that the observations are very certainly opposed to independence f sign of charge and sign of field.

Although, most often, the sign of drop charge was opposed to that of the eld, there were nearly always some drops of the same sign as the field. Indeed, n one occasion, during light continuous rain with little wind and a field of −300 V/m large number of charges was observed in a short time, all negative, between -0·5 × 10^{-3} esu and −1·0 × 10^{-3} esu.

(b) *Results with point discharge*

In general charges were much higher when point discharge occurred than herwise. Taken from all the periods of rainfall examined, there were 173 drops or which both the radius a and the charge Q were known as well as the simultaneous alue of the point-discharge current I. These are analysed in Table II.

It is not practicable to tabulate all the results obtained, but the 173 drops ere arranged in 6 groups, according to their radius, using intervals of 0·2 mm, nd the average values of $Q/-I$ for each group are plotted against a in Fig. 3. The umbers against each point signify the number of drops in the group.

TABLE II.

Drops with positive charge	67
Drops with zero charge (to the limit of measurment)	31
Drops with negative charge	75
Q and I of same sign	37
Q and I of opposite sign	105
Range of values of a	0·09 mm to 1·61 mm
Range of values of Q	$-67{\cdot}5 \times 10^{-3}$ ESU to $+49{\cdot}5 \times 10^{-3}$ ESU
Range of values of I	$-3{\cdot}5$ μa to $5{\cdot}0$ μa

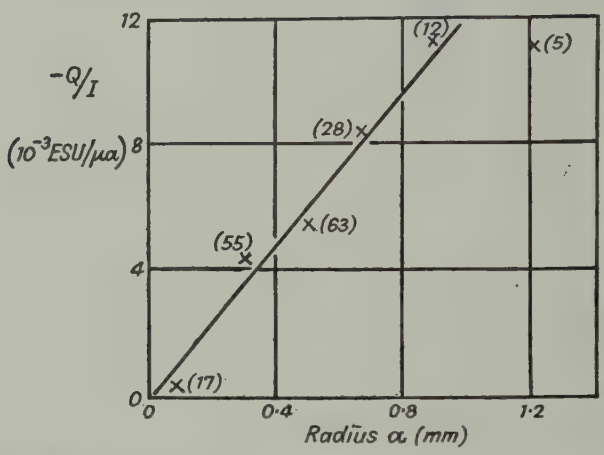

Figure 3. Relation between $-Q/I$ and a for average values (figures denote numbers of drops).

Partial correlation coefficients were evaluated for the various pairs of variables and also the total correlation coefficient of $Q/-I$ against a. The results are shown in Table III.

TABLE III.

Variables	Correlation coeff.	Probability
a, Q	$-{\cdot}15 \pm {\cdot}07$	4·5 in 100
a, $-I$	$+{\cdot}05 \pm {\cdot}08$	55 in 100
Q, $-I$	$+{\cdot}36 \pm {\cdot}07$	1 in 10^6
$Q/-I$, a	$+{\cdot}18 \pm {\cdot}07$	1·5 in 100

The *probability* given in the last column of the table is the statistical probability of such a correlation coefficient occurring by chance with 173 observations. The conclusions to be drawn are :

1. A physical relationship between Q and $-I$ is established.
2. There is a probable relationship between $Q/-I$ and a.

3. There is a possible relationship between $-Q$ and a, meaning that the largest drops may tend to carry negative charges.

4. No relation between a and $-I$ is apparent.

The relationship between Q and I is particularly noticeable for measurements on 3 June 1948. Rain was continuous during the morning and afternoon at rates between 0·04 and 0·16 in./hr, and at times there was sustained point discharge, both signs occurring. In one particular period of 24 min, commencing at 1106 hr, there were 37 drops for each of which Q and a were measured, any drops suspected of having broken being rejected. The drop radius a was nearly constant (average 0·48 mm). The values of Q and I are shown in Fig. 4 lying mostly close to a

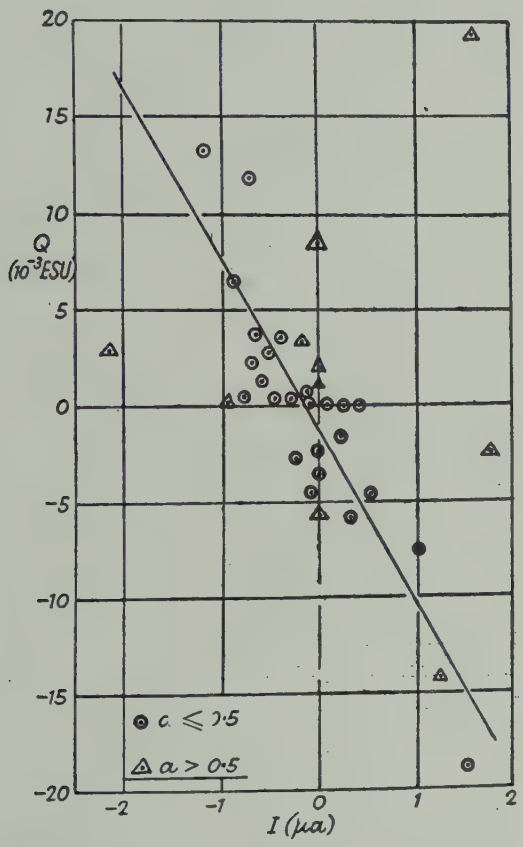

Figure 4. Drop charge Q and point discharge current I for the period beginning 1106 hr, 3 June 1948

straight line which passes near the origin, in agreement with the general conclusion above.

In all measurements, the value of I used is the instantaneous value at the time the drop arrived ; if average values of I over a period of 1 min or longer before

arrival were used, Fig. 4 would show no better linearity and no improvement in correlation is found.

If the magnitude, but not the sign, of Q is considered, this is found to increase as a increases, but no definite power law can be established, though Fig. 3 suggests $Q \propto a$ if I and a are independent.

4. *Comparison with other results*

The present results agree with those of Gschwend (1922) and Chalmers and Pasquill (1938) in showing that in nearly all rain there is a mixture of positive and negative charges. At the same time (as was found by Gschwend) the signs of drop charges and field are more often opposite than alike. Gschwend did not find any definite relation between drop charge and field or size of drop, so that no further comparison is possible.

The only other series of observations relating electric charge on rain with point discharge is that of Simpson (1949) who measured charges, not on single drops, but on the volume of rain arriving at an area of the earth's surface in a given time. He also found a direct relation between the rain charge and $-I$, and found that this increased with the rate of rainfall. A comparison between the present results and those of Simpson is possible by making use of the fact that there is a relation between the rate of rainfall and the average size of the drops in the rain. Best (1943) has given the relation $D_v \propto R^{1/4}$ based on various results, connecting D_v, the diameter of the drop whose volume is the average of that of all the drops, with R, the rate of rainfall. By converting Simpson's rates of rainfall into corresponding average radii, one can obtain a relation between $Q/-I$ and a from Simpson's results ; this turns out to be very similar to that of Fig. 3 except that the ordinates are smaller by a factor of about 8. It is to be expected that what influences Q is not so much I, the current through one single point as the point-discharge current density, and so it is reasonable that different single points should differ in effectiveness in contributing to the point-discharge current density. If, therefore, Simpson's point is about 8 times as effective as that used at Durham, the results are brought into harmony. The agreement of the variation of Q/I with a between the present results and those deduced from Simpson's can increase the confidence to be placed in the conclusions of Fig. 3.

The spread of the individual results is bound to be greater in the present work than in Simpson's for two reasons. In the first place, there is a natural averaging already present in Simpson's results, since each observation comprises a large number of individual drops ; also Simpson selected periods in which the conditions were fairly steady and he rejected all periods in which the rain current and point-discharge current were of the same sign. In the present work, no such selection or rejection has been made and every observation has been included if it appeared genuine. The only cases rejected were those where it seemed probable that the drop broke on the cone, or where it has been uncertain to which of two drops a charge should be attributed.

Gunn (1949) who measured Q and a for 159 drops in 3 thunderstorms found an approximate relation for the average, irrespective of sign, of $Q = 2{\cdot}8\ a^2$. The

resent results give Q/a^2 varying from 0 to 49, with an average value of about 3·0. This shows that the present results, for all types of rain, when point discharge ccurs, do not differ markedly from those of Gunn during active thunderstorms. Gunn did not measure point discharge, so that no further comparison is possible.

5. Discussion

For rain without point discharge, so many of the drops have charges less than he limit of sensitivity of the amplifier that no conclusions can be drawn except hat the charge and field are usually of opposite sign.

In the case of rain during point discharge, it is very significant that the relation-hips apply to rain charges and point-discharge currents of either sign, observed n roughly equal proportions, and a similar conclusion was reached by Simpson 1949). Fig. 4 for 3 June 1948 shows this very clearly. To account for such esults, some reversible process is necessary giving positive charge in negative fields nd negative charge in positive fields.

The outstanding feature of the observational results, that the rain charge is usually of opposite sign to the point-discharge current, and hence to the field, night arise in two ways. Either (a) the field is first set up, and then the raindrops acquire charge from the ions produced by point discharge, or (b) a separation of charge occurs, in the cloud or below, the rain bringing down charge of one sign, while the charge remaining behind sets up the field of opposite sign.

If the origin of the relation is of type (a), then we should expect the rain current density to be less than the point-discharge current density, since the rain could acquire only part of the charge in the point-discharge current.

On the other hand, if alternative (b) is correct, the rain-current density would be expected to be greater than the point-discharge current density. If the separation of charge produces positive rain and leaves negative charge behind, then we should expect part of this negative charge to be dissipated upwards and thus only part could remain or move downwards to be neutralized by the point-discharge current. Thus, if conditions are steady and the separation process produces equal quantities of both signs of charge, the rain-current density would exceed the point-discharge current density.

These alternative possibilities were put forward by Chalmers and Little (1940) in discussing a particular case of rain current and point-discharge current* of opposite sign ; but at that time it was not possible to decide between the alternatives. The measurement of point discharge is always of that through a single point only, and the point discharge current density cannot be obtained unless we know the effective separation of similar points.

Simpson's results provide some evidence in favour of alternative (a). He found indications that the ratio of rain current to point-discharge current approached a limit for very high rates of rainfall and suggested that this would be accounted for if the heavy rain could acquire nearly the whole of the point-discharge current ; no such explanation is feasible with alternative (b). Simpson also found that there is a clear distinction between the relations of rain current to field with and without

* It should be pointed out that the discharging point used in the present work is not in the same situation as that used by Chalmers and Little.

point discharge. This is also in favour of alternative (a), in which the point-discharge ions would affect the rain charge, while with alternative (b) this would not be so.

Simpson has pointed out that his results appear at first to be in general agreement with the ion-capture process first suggested by Wilson (1929), according to which a drop of water falling in a vertical electric field will be polarised and will be able to capture ions preferentially. The theory, worked out in detail by Whipple and Chalmers (1944), shows that the maximum charge on a drop of radius a in a field X, with ions of one sign only present, is $-3\,X\,a^2$. It is therefore of interest to compare the observed drop charges with the corresponding values of $-3\,X_0\,a^2$, where X_0 is the field at the ground. For this purpose we consider the 173 drops measured ; eliminating the 33 drops for which Q and X_0 are of the same sign, the remainder are tabulated in Table IV (measured values of field, and not of point discharge, have been used).

TABLE IV. Comparison of charge with theoretical maximum

Values of $Q/(-3X_0a^2)$	Number of drops
0	31
0—1	5
1—2	10
2—5	32
5—10	31
>10	31

This shows conclusively that the drops cannot have acquired their charge by Wilson's process if the field is everywhere the same as at the ground. This conclusion is more noteworthy than that of Simpson (1949) to the same effect, since Simpson had to make assumptions in regard to average drop size. If the drops do, in fact, obtain charges by Wilson's process, then clearly they must have fallen through fields several times larger than the field X_0 at the ground. As Simpson points out, this is in agreement with the idea of a " blanket " space charge between clouds and earth, as discussed by Wilson (1925) and Whipple and Scrase (1936).

Further discussion of the origin of the relation between Q and I and any discussion of dependence on a is left to a later publication.

6. Conclusion

The general results show that the raindrop charge and the field are usually opposite in sign. For rain without point discharge, a more sensitive amplifier is needed. For rain with point discharge, the rain charges are opposite in sign to the point-discharge current and the ratio $Q/-I$ probably increases with the size of drop. There is also some indication that the largest drops tend to have negative charges.

Acknowledgments

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A study of cold fronts over the British Isles

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SUMMARY

Fifty cold fronts are classified by means of two simultaneous aerological soundings. Two general categories are found :

(1) Those in which the warm air is descending, usually forming a subsidence inversion. The name *Katafront* (following Bergeron) is applied to such fronts.

(2) Fronts at which the warm air is ascending. These, which are named *Anafronts*, usually show no sharp discontinuity on a tephigram.

The two types of cold front give rise to quite different weather conditions. The changes in wind and temperature are generally much more abrupt at an anafront, where there is also much more precipitation. A katafront frequently brings a rapid clearance followed by fine weather.

The variation of wind with height through the front is markedly different at the two types, the chief characteristics being :

Anafront : Rapid backing of wind with height ; the component normal to the front decreases. The upper winds are inclined at a small angle (about 15°) to the line of the front.

Katafront : Slight backing with height ; the normal component increases considerably. The upper winds are inclined at a large angle (about 40°) to the front. Consideration of the horizontal wind changes across the front leads to the expectation of troughs with strong cyclonic shear at anafronts, and the more rapid occlusion of katafronts.

Anafronts and katafronts are generally associated with different stages in the history of a cold front ; an anafront is normally the initial state, but develops into a katafront later as the depression becomes more occluded. Katafronts are the more common type in this country, especially in summer.

Calculations of the mean slope above the friction layer give values of about 1/70 for anafronts, and 1/300 for katafronts. The vertical velocity of the warm air at an anafront is of the order of 10 cm/sec upwards, while at a katafront it is about 2·5 cm/sec downwards. Vertical cross-sections of wet-bulb potential temperature show that there is often latent or convective instability at the nose of a katafront. This may be the explanation of the large ascending velocities found in this region by sailplane pilots.

1. INTRODUCTION

Meteorologists have for many years recognised the existence of two main types of cold front according to the vertical motion occurring in the two air masses, but unfortunately there is no universal nomenclature. Bergeron (1937) suggested the introduction of the names *anafront* and *katafront* to distinguish between fronts at which there was a general upsliding of the warm air, and those at which descent occurred in all but the lowest layers. These terms have not come into general use, but for the sake of convenience they will be adopted and used throughout this paper. Although various writers have tried to indicate the conditions under which the different types of cold front should be found, there does not seem to have been any attempt to relate the motion and behaviour of the cold front with the distribution of wind, pressure and temperature. J. Bjerknes, Palmén and others have examined a few selected cyclones in great detail, but no general conclusions can be reached from these rather isolated cases. Palmén (1946) has also theoretically considered certain problems of frontal analysis in the free atmosphere,

but the chosen case was rather hypothetical, with parallel north-south warm and cold fronts and a barotropic field of mass, and so this cannot materially help to establish any relation between frontal behaviour and the distribution of pressure *etc.* in the upper air.

The development during the war of a network of radiosonde stations reporting upper air temperatures, humidities and winds every 6 hr has now made possible the investigation of a reasonable number of cold fronts in detail. This has been undertaken in the present paper, which is based on a statistical and detailed individual survey of 50 cold fronts. The fronts were divided into anafronts and katafronts and the associated weather conditions and synoptic situations were studied. In this way many differences have been found, and it has been possible to put forward a probable life cycle for cold fronts. Various earlier statements concerning the structure, behaviour and occurrence of cold fronts have also been carefully examined and tested.

2. Procedure adopted

A selection was made during the years 1944-8 of 50 cold fronts which crossed the British Isles. This represents about one in six of all cold fronts drawn on the *Daily Weather Report* during this period, and it is believed that the chosen sample is fully representative of all cold fronts passing over this country. An analysis of the various situations indicates that these fifty cases cover a wide range of conditions likely to be encountered over the British Isles. The time for examination of each front was chosen so that the surface front was positioned with two radiosonde stations 50 to 150 mi on either side, and with the line joining them as nearly as possible normal to the front. No restriction was placed on the selection of any cold front, other than that approximately simultaneous soundings should be available from the two stations, and that these should be free from the influence of any other front.

The soundings from the station ahead (1) and behind (2) the front were plotted on a tephigram for each of the fifty cases. Before many fronts had been studied it was apparent that there were two distinct forms taken by the sounding 2. The most frequent occurrence, provisionally called type A, was a fairly sharp inversion or isothermal layer with very low humidities at higher levels. Other fronts (type C) showed no sharp temperature discontinuity, and the relative humidities were generally high at all levels. A simple classification, with an arbitrary criterion for each category, was devised to bring out this difference. The horizontal difference between the humidity mixing ratios (r_1, r_2) in the two soundings was found to be most suitable for this purpose. The limits for type A were set at $(r_1 - r_2)/r_1 \geqslant 0{\cdot}65$ for a layer at least 200 mb deep above the base of the temperature discontinuity ; and for type C, $(r_1 - r_2)/r_1 \leqslant 0{\cdot}30$ for a similar layer accompanied by a reduced lapse rate. Twelve fronts which did not fall into either category were provisionally labelled type B, but these were only a minority group, as 23 cold fronts were classed as type A and 15 as type C. The essential differences between these fronts can be seen in Fig. 1 which shows mean values of ΔT and $\Delta r/r_1$ (actually $\Sigma\Delta r/\Sigma r_1$) at each level for the two main categories. The values for type B, which are not reproduced, are of course always intermediate between those for the other categories.

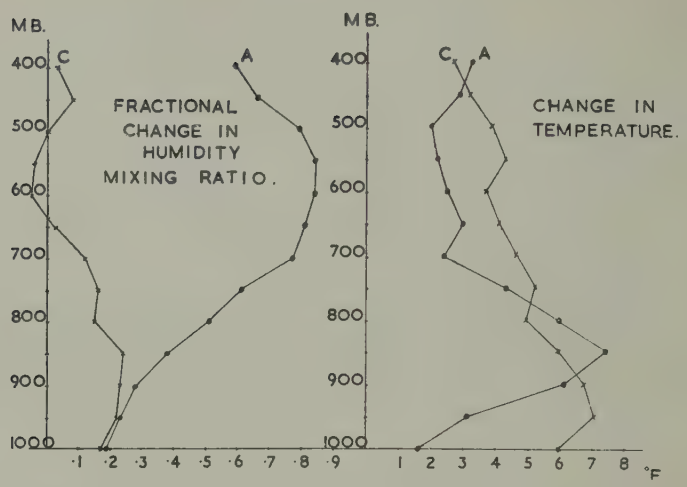


Figure 1. Mean values of ΔT and $\Sigma \Delta r / \Sigma r_1$ at type A and C cold fronts.

3. Anafronts and katafronts

(a) *The significance of the classification*

The inversion of the type A front was first described by Giblett (1923) and discussed by Douglas (1929). The base of the inversion usually lies in the cold air, as can be seen by means of the wet-bulb potential temperature, and the transition from cold air to warm is usually gradual, taking place within the very dry air above the inversion. It is clear that the inversion is primarily due to subsidence which is generally occurring in both air masses. It is proposed that the name *katafront*, originally suggested by Bergeron (1937), be given to these fronts. The prefix *kata* applies essentially to the warm air, but will in most cases be equally true of the cold air.

The type C front is the result of ascent of the warm air mass, giving generally saturated air above the front. The motion of the cold air is less clearly determined at this stage, but it will be seen later from a study of the winds that it too is generally ascending. Following Bergeron again, the type C front will in future be referred to as an *anafront*.

The unclassified type B fronts are frequently a transitional stage, or fronts which could be described as either weak anafronts or katafronts. In all the statistical analysis of this paper they are, however, retained as a separate category in order to emphasise the differences between well-marked ana- and katafronts. If these fronts were split up between the other categories one would find that katafronts accounted for about 60 per cent. of the cold fronts in this country on the whole, but Table I shows that there is a strong seasonal variation, anafronts being extremely rare during the summer months.

TABLE I. Seasonal distribution of cold fronts

	Winter Dec. – Feb.	Spring Mar. – May	Summer June – Aug.	Autumn Sept. – Nov.
Anafronts	8	5	0	2
Katafronts	1	3	10	9
Unclassified	3	4	1	4

(b) *Weather conditions*

The changes in the important meteorological elements which occur at the surface with the passage of a cold front of each type are briefly summarized below :

ANAFRONT

Temperature : often a large fall, which may be very sudden.
Relative humidity : high ; changes slight.
Cloud : slow clearance ; cloud structure resembles that of a warm front in reverse.
Precipitation : generally fairly heavy rain at frontal passage with steady rain for some time behind front.
Wind : usually a sharp veer, accompanied by a marked *decrease* in wind speed.

KATAFRONT

Temperature : changes may be very slight and gradual.
Relative humidity : decreases ; the fall may be of considerable magnitude and quite sharp.
Cloud : usually a rapid and often complete clearance.
Precipitation : amounts generally very slight, and frequently nil.
Wind : wind veer may be very gradual, and speed changes usually slight.

Some of these differences noted above are worthy of a little further consideration and explanation.

i) *Temperature.* It will be seen from Fig. 1 that the mean temperature difference across an anafront near the surface is about 6°F, while for a katafront it is only 1·5°F. At 850 mb however, it is the katafront which has the larger difference, namely 7·5°F, while the anafront again gives 6°F. This can best be explained by assuming that at 850 mb both air masses are subsiding at a katafront, with a greater rate of descent occurring in the warm air. The temperature contrast between the two air masses would thus be increased by the greater adiabatic warming of the warm air. Near the surface, on the other hand, the warm air just ahead of the front rises while the cold air continues to sink, and in this case adiabatic warming of the cold air will tend to destroy the temperature contrast between the two air masses.

ii) *Rainfall.* A comparison between the rainfall at anafronts and katafronts was made by examining the hyetograms for six stations on each of 14 frontal situations (seven of each type). It was found that the average rainfall per station with the anafront was 6·5 mm, but at a katafront it was only 0·8 mm. At anafronts most

of the rain is postfrontal, usually with a short period of heavy rain followed by a longer period (2 to 3 hr) of lighter steady rain. The katafront, on the other hand, gives very little postfrontal rain ; the precipitation occurs immediately before or during the passage of the front.

(iii) *Wind speed.* Cold fronts of both types are generally accompanied by a decrease in wind speed ; this is especially the case with anafronts, two-thirds of which showed a quite sharp fall in speed. By studying the surface isobars at each front the sign of the change in geostrophic wind speed was determined, and the results are given in Table II.

TABLE II. CHANGE IN GEOSTROPHIC WIND SPEED AT A COLD FRONT

	Increase	Steady	Decrease
Anafronts	1	4	10
Katafronts	3	10	10

It seems clear that at an anafront the isobars in the warm sector make a smaller angle to the line of the front than those in the cold air.

Many of these features of the weather at cold fronts are clearly exemplified in the surface charts for a typical anafront (3 January 1945) and katafront (1 March 1945), and in the autographic records from Kew Observatory depicting the passage of the cold front on the same dates ; these are reproduced as Figs. 6 and 7 in Section 8.

4. WIND STRUCTURE AT A COLD FRONT

(a) *Vertical structure*

A wind sounding passing up through the frontal surface was analysed for each cold front, and mean values of the wind components normal and parallel to the front were obtained for each category. These are shown in Fig. 2, on which the mean speed of the front for each category is also indicated. The following main features should be observed :

(i) *Anafront*

a. The normal component of the wind decresses slightly while the parallel component increases rapidly with height.

b. The total backing of the wind from 950 mb to 400 mb is about 65°.

c. At 500 mb the wind direction is inclined at only 16° to the tangent to the front.

d. The thermal wind between 950 mb and 400 mb is almost parallel to the front (actually —3·5°) and corresponds to a thermal gradient of 1·25°F per 50 km.

e. The mean speed of the front exceeds the mean normal component of the wind at all heights, though the difference is insignificant below 800 mb. Higher up the difference is definite, which implies that both warm and cold air masses are ascending.

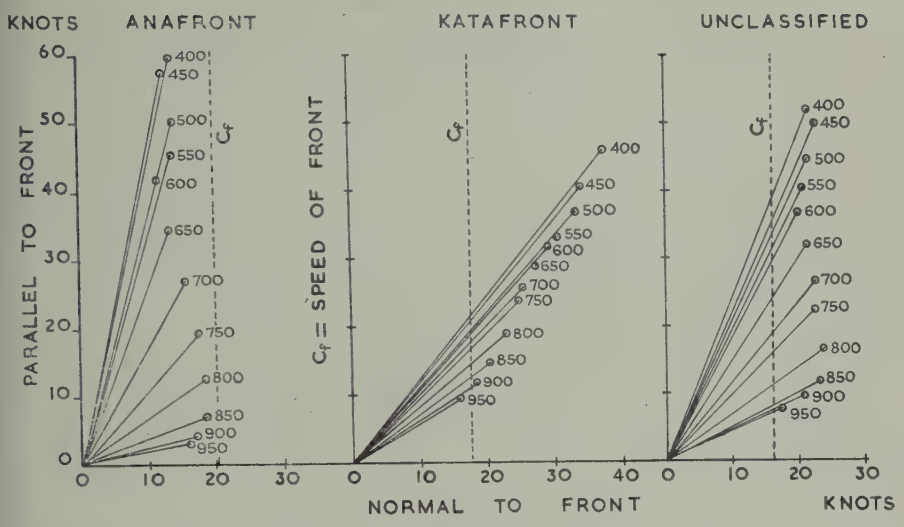


Figure 2. Variation of wind with height through a cold front.

ii) *Katafront*

a. Both the normal and parallel components of the wind increase with height.
b. The total backing from 950 mb to 400 mb is only 20°.
c. At 500 mb the mean wind direction is at an angle of 42° to the tangent to the front.
d. The thermal wind is inclined at 30° across the front and corresponds to a temperature gradient of 0·9°F per 50 km. This cross-front thermal wind is an essential condition for an active katafront, and can be readily recognized.
e. The normal component of the wind exceeds the speed of the front at all levels above the lowest layers, showing that both air masses are generally subsiding.

iii) *Unclassified fronts.* The mean wind-height diagram is generally intermediate in character between those for anafronts and katafronts, with the normal wind component almost constant with height. Since the average speed of these fronts is less than the mean normal wind component it appears that the majority of the fronts are weak katafronts.

iv) *Warm-sector winds.* The winds in the warm sector 50 to 150 mi ahead of the front were also examined. There is very little change of direction with height, but the actual angle between the warm sector winds and the front differs for the two categories. At an anafront this is always small, whereas at a katafront it is about twice as great. The actual angles for both types of cold front, and for the unclassified fronts, at 700 mb and 500 mb are given in Table III.

TABLE III. ANGLE BETWEEN MEAN WIND DIRECTION IN WARM SECTOR AND THE TANGENT TO THE COLD FRONT

	Anafront	Katafront	Unclassified
700 mb	18°	35°	24°
500 mb	17°	35°	19°

At an anafront the warm air is moving slower than the front at all levels and is consequently ascending ; while at a katafront the air at high levels is moving faster than the front and descending, but near the surface the mean speed is less than that of the front and so the warm air here is generally rising.

Application of wind-height diagrams to the practical forecasting of rainfall at fronts has recently been suggested by Parker (1949). Use of this method will always distinguish between anafronts and katafronts, and will therefore indicate the probability of rainfall. However it seems clear that accurate calculations of the speed of the vertical currents or the rate of rainfall cannot be made by consideration of the normal component of the wind alone. The tangential component, which can be very important at an anafront, may also vary along the front and so contribute considerably to the vertical motion. The simple two-dimensional picture of a cold front with the cold air undercutting the warm air and subsiding, while forcing the warm air to ascend, is not really adequate to explain the motion.

(b) *Horizontal wind structure*

If there is vertical motion occurring along a sloping frontal surface, there must also be a discontinuity in the horizontal wind component across the front. J. Bjerknes (1924) related the horizontal wind changes at a front with the corresponding changes in acceleration, and the results are shown in Fig. 3.

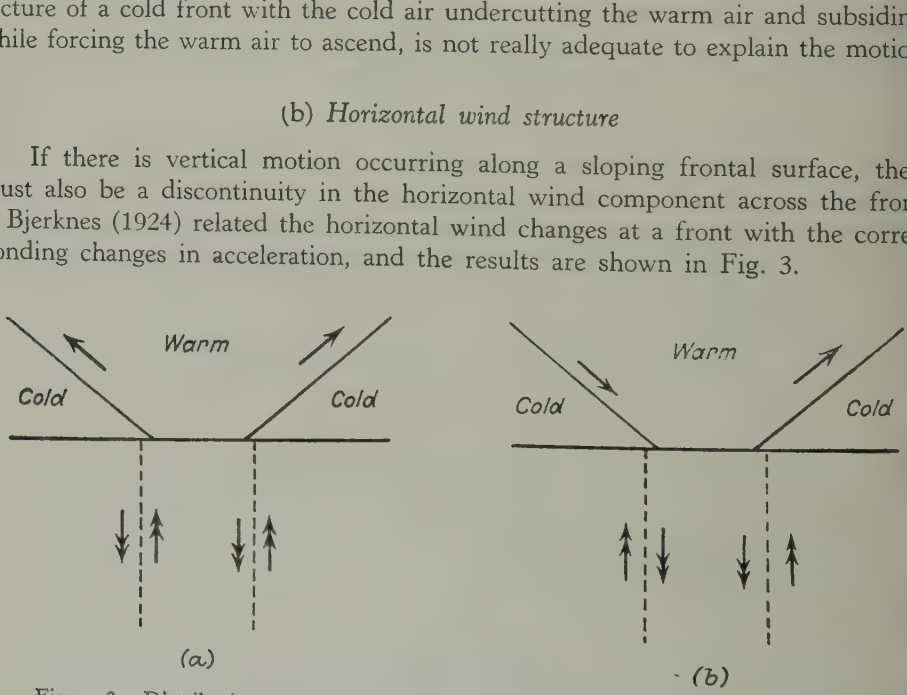


Figure 3. Distribution of velocity and acceleration at a warm-sector frontal system.
(a) Cold front shown as anafront.
(b) Cold front shown as katafront.
The upper part of the diagram is a vertical section, and the lower part a horizontal section through the warm sector. Single arrows denote velocity of warm air relative to the adjacent cold air ; double arrows denote accelerations in horizontal plane. (After J. Bjerknes, 1924)

The relation between the horizontal wind speed and acceleration can most simply be seen by referring the motion to geostrophic flow, which is, by definition, unaccelerated. If the wind speed is greater than geostrophic the air will be accelerated to the right ; and if the speed is less than geostrophic, it will be accelerated to the left ; and the greater the departure from geostrophic, the greater will be the acceleration.

If the speeds of the cold front and of the cold air are approximately given by the geostrophic equation, then it is clear that, at an anafront, the warm air will be moving with less than geostrophic speed, and so will be accelerated towards the left (Fig. 3a). The relative accelerations shown in Fig. 3 would be unchanged even if the geostrophic departure occurred in the cold air, so the argument is valid in all cases. By similar reasoning it can be seen that the accelerations are reversed at a katafront. (Fig. 3b).

The accelerations produced at an anafront thus tend to increase the cyclonic wind shear at the front, while those at a katafront oppose it. Well-developed troughs in the pressure field, both at the surface and aloft, are therefore mainly associated with anafronts, as can be seen from Tables IV and V. Table IV shows the frequency of occurrence at the surface of *sharp* V-troughs (with large wind veer), *average* troughs (with moderate wind veer), and *weak* troughs (with slight wind veer).

TABLE IV. Frequency of occurrence of *sharp*, *average* or *weak* troughs at the surface

	Sharp	Average	Weak
Anafronts	7	2	6
Katafronts	2	8	13

Table V, referring to the upper air, gives the average, for all cases, of the total wind veer following the passage of a front.

TABLE V. Average upper wind veer at passage of front

	700 mb	500 mb
Anafront	23°	15°
Katafront	15°	5°

Fig. 3(b) also shows that when the warm air is subsiding at the cold front and ascending at the warm front, the accelerations developed will lead to the rapid *eviction* of the warm sector (Gold 1935), and so to the more rapid occlusion of the depression. The katafront should therefore be more frequently found with occluding depressions, an inference which is clearly confirmed by examination of

surface charts. Only 4 anafronts out of 15 had started to occlude, and in 2 of these further occlusion was taking place only very slowly, whereas 13 out of 23 katafronts were already partially occluded, with further occlusion proceeding quite rapidly.

5. The vertical structure of a cold front

(a) *The slope of the front*

By using Margules' formula for the inclination of a frontal surface it is possible to calculate the mean value for the slope of anafronts and katafronts (neglecting acceleration and friction) from mean values of the wind, velocity and temperature in the two air masses.

$$\tan\theta = \frac{2\omega \sin\phi}{g} \frac{T'v - Tv'}{T - T'} \quad . \quad . \quad . \quad . \quad (1)$$

$$\simeq \frac{2\omega \sin\phi}{g} T_m \frac{\Delta v}{\Delta T} \quad . \quad . \quad . \quad . \quad (2)$$

where ϕ is the latitude, T, T' are the temperatures, v, v' the wind components parallel to the front, and $T_m = \frac{1}{2}(T + T')$. Primed symbols refer to the warm air.

Taking $\phi = 52°$, $T_m = 273°K$, and expressing Δv in m/sec, ΔT in °C, we get :

$$\tan\theta \simeq 3{\cdot}26 \times 10^{-3}\, \Delta v/\Delta T \quad . \quad . \quad . \quad . \quad . \quad (3)$$

At about 850 mb the effect of surface friction is negligible and since at this level the mean temperature difference between the two soundings is at its maximum (Fig. 1), the mean winds from the same two soundings should also be fully representative of the two air masses in the neighbourhood of the front. The results obtained are shown in Table VI.

TABLE VI. Calculation of the average slope of cold fronts

Pressure Mb	ΔT °C	v' m.sec^{-1}	v m.sec^{-1}	Δv m.sec.$^{-1}$	$\Delta v/\Delta T$	$\tan\theta$
			Anafronts			
900	3·7	16·4	2·3	14·1	3·81	
850	3·3	18·4	3·8	14·6	4·42	1/80
800	2·7	18·9	6·7	12·2	4·51	1/70
						1/70
			Katafronts			
900	3·8	10·7	6·0	4·7	1·24	
850	4·1	11·7	7·5	4·2	1·02	1/245
800	3·3	12·6	9·7	2·9	0·88	1/300
						1/350
			Unclassified			
900	3·1	12·5	4·9	7·6	2·45	
850	3·6	13·5	6·1	7·4	2·05	1/125
800	4·1	15·2	8·6	6·6	1·61	1/150
						1/190

t 850 mb the mean slope of anafronts is 1/70, katafronts 1/300, and unclassified onts about 1/150. It appears from the Table that the slope of both the katafronts nd the unclassified fronts decreases with height even above the friction layer ; 750 mb the anafront slope also has begun to decrease and has fallen to 1/95. is not however possible to calculate values for higher levels by this method as ie two soundings would be no longer representative of the two air masses. The ery small slopes calculated for katafronts, which are the most common cold fronts this country, seem to be in conflict with the commonly assumed average slope f 1/50 to 1/100, but it should be realized that these calculated slopes give the ctual value at a particular height, and not the mean value from the ground up that height, to which the other values refer. It seems that many katafronts e very steep along the lower forward part of the front and it has been shown Firesah 1939) that this steep nose may extend to a height of several kilometres. xamples of this can be seen in Figs. 4 and 11 (b).

(b) *Vertical velocities*

The equation frequently quoted for the relation between the vertical velocity, , the slope of the front, tan θ, and the speed of the front, c, is

$$w = (c - u) \tan\theta \quad . \quad . \quad . \quad . \quad . \quad (4)$$

here u is the horizontal component of the air motion normal to the front. Since his is based on conditions of steady, two-dimensional flow it cannot be used to btain accurate values of the vertical currents at a front, though it will give the eneral order of magnitude. Substitution into this equation of general values rom Fig. 2 results in a value of $w = 5$ cm/sec upwards for anafronts ; this is robably well below the true value. At katafronts one obtains $w = -2{\cdot}5$ cm/sec, nd this is clearly of the right order for descending currents.

There are several methods by which the vertical component can be calculated n a particular case ; at an anafront the best method is that suggested by Bannon 1948) using the rate of rainfall. It is also possible to follow a trajectory and calculate he vertical motion from the distribution of potential temperature or wet-bulb otential temperature at the starting and finishing points of a 6 or 12 hr run. Eq. (4) an also be applied to individual cases, but as already mentioned the results may ot be very reliable. Each of these three methods was applied to a well-defined nafront (January 3 1945). Substitution of the appropriate values into Eq. (4) ave $w = 14$ cm/sec ; from a twelve-hour trajectory at about 700 mb, a vertical peed of about 4 cm/sec was computed ; the rate of rainfall at Kew Observatory vas about 5·4 mm/hr for the first 25 minutes, and thereafter about 1 mm/hr (see ig. 7) ; these correspond to vertical velocities of about 25 cm/sec and 7 cm/sec espectively. Miles (1949) also made comparisons for another anafront (25 October 948) obtaining very similar results. It seems that the trajectory method is rather nreliable, and it can frequently be ambiguous, unless there are ample upper air oundings. Computations were also made for the katafront of March 1 1945 ; sing a trajectory at 700 mb over a period of six hours a rate of subsidence of 2·5 m/sec was calculated, while substitution into Eq. (4) resulted in a value of about cm/sec. The rainfall method cannot, of course, be applied to katafronts.

(c) *Cold fronts and soaring flight*

It has long been recognized by experienced sail-plane pilots that a cold front sometimes gives exceptional facilities for soaring flight. Since the sinking flight of a normal sail-plane is about 75 cm/sec, the mechanical lift at a cold front is quite insufficient to support it. Pilots have, however, reported lift of the order of 350 to 450 cm/sec on some cold fronts, and a figure around 200 cm/sec is frequently quoted. This area of high lift is found just in advance of the frontal cloud (although even higher values may exist within the cloud), and is usually associated with very calm flying conditions. The uniformity and absence of bumpiness just ahead of the cloud seems to preclude any purely convective origin for these currents. Rainey (1947), describing a cold front flight in S. Africa, has suggested that the effect may be explained in terms of the localization of the previously scattered convection currents into a narrow zone just ahead of the advancing cold air. While this effect may be taking place, and there is certain visual evidence to support the hypothesis, no explanation has been offered of why or how this is brought about. Firesah (1939) found that in a vertical section of a cold front the lines of equal wet-bulb potential temperature frequently showed a nose raised some kilometres above the ground ; up to this level the wet-bulb potential temperature decreases with height, and the air in this region possesses convective or latent instability which can be realized after only relatively small upward displacement. The cold front discussed by Firesah is a katafront (December 26 1928) and the same phenomenon has been found with several katafronts of the present investigation. It can be clearly seen in the cross-section for 24 April 1946*, Fig. 4, and for 1 March 1945, Fig. 11(b). It has not been apparent on any cross-sections of anafronts. Rainey's cold front was also evidently a katafront and it seems that this type may offer the best facilities for soaring. The question of whether cold front flights are possible at night or over the sea has often been discussed by sail-plane pilots. On Rainey's

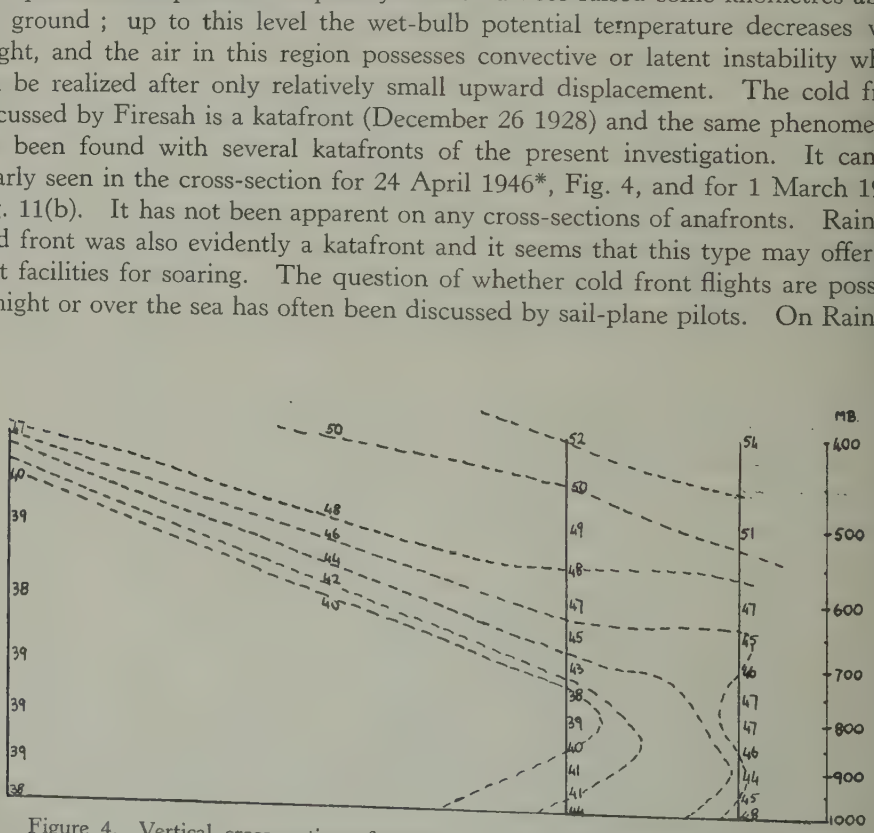


Figure 4. Vertical cross-section of wet-bulb potential temperature at a katafront.

* This front was actually very narrowly rejected from the original type A category (katafront) by the arbitrary criterion chosen, but it is, nevertheless, a good example of a katafront.

onvective hypothesis this would seem improbable, but Firesah's work revealing he convective instability at the nose of the front suggests that, provided the nechanical lifting is sufficient to release this instability, there should be almost qually good lift at night as in the day ; the instability is certainly still present at nidnight as the cross-section in Fig. 4 shows.

6. The life cycle of cold fronts

Three possible relationships between anafronts and katafronts must be considered , (a) the two types are totally independent ; (b) there is a gradual transition along the length of the front ; (c) there is a gradual transition in time as the cyclone develops.

(a) *Independent existence*

In certain cases it does appear that a cold front may remain as a katafront or an anafront throughout its existence without showing any tendency for transition. This is most likely to occur when a small secondary depression has formed on a cold front within the upper circulation of the primary. In this case the cold front of the secondary will usually be a katafront, as the upper winds have a large component across the front. A good example of such a katafront was seen on 10 November 1947, but this is by no means the normal state of affairs, and in general there is some form of transition between the two types.

(b) *Transition along the front*

Many text-books support the view that there is transition along the front, and some (*e.g. The Handbook of Meteorology*, Berry, Bollay & Beers) state implicitly that the anafront is the type of cold front occurring outside the zone of cyclonic activity, whereas the katafront is found nearer the centre of the cyclone. This statement was accordingly tested, and no evidence in support of it was obtained ; on the contrary, the facts support, if anything, the opposite viewpoint, as is shown by Table VII. The small number of cases available, however, makes generalization of little value here.

TABLE VII. Frequency of observation of cold fronts at different values of surface pressure

	below 1005 mb	1005 – 1015 mb	over 1015 mb
Anafronts	8	4	3
Katafronts	7	7	9

The difference in pressure between the centre of the cyclone and the portion of the front examined was also recorded for each anafront and katafront. Table VIII shows how these cases were distributed. There is certainly nothing in these figures to indicate that one type is more prevalent further from, or nearer to, the centre of the depression.

TABLE VIII.—Pressure difference between the centre of the cyclone and the point at which the cold front was examined

	0 – 15 mb	16 – 30 mb	over 30 mb
Anafronts	5	7	3
Katafronts	6	12	5

(c) *Transition with development*

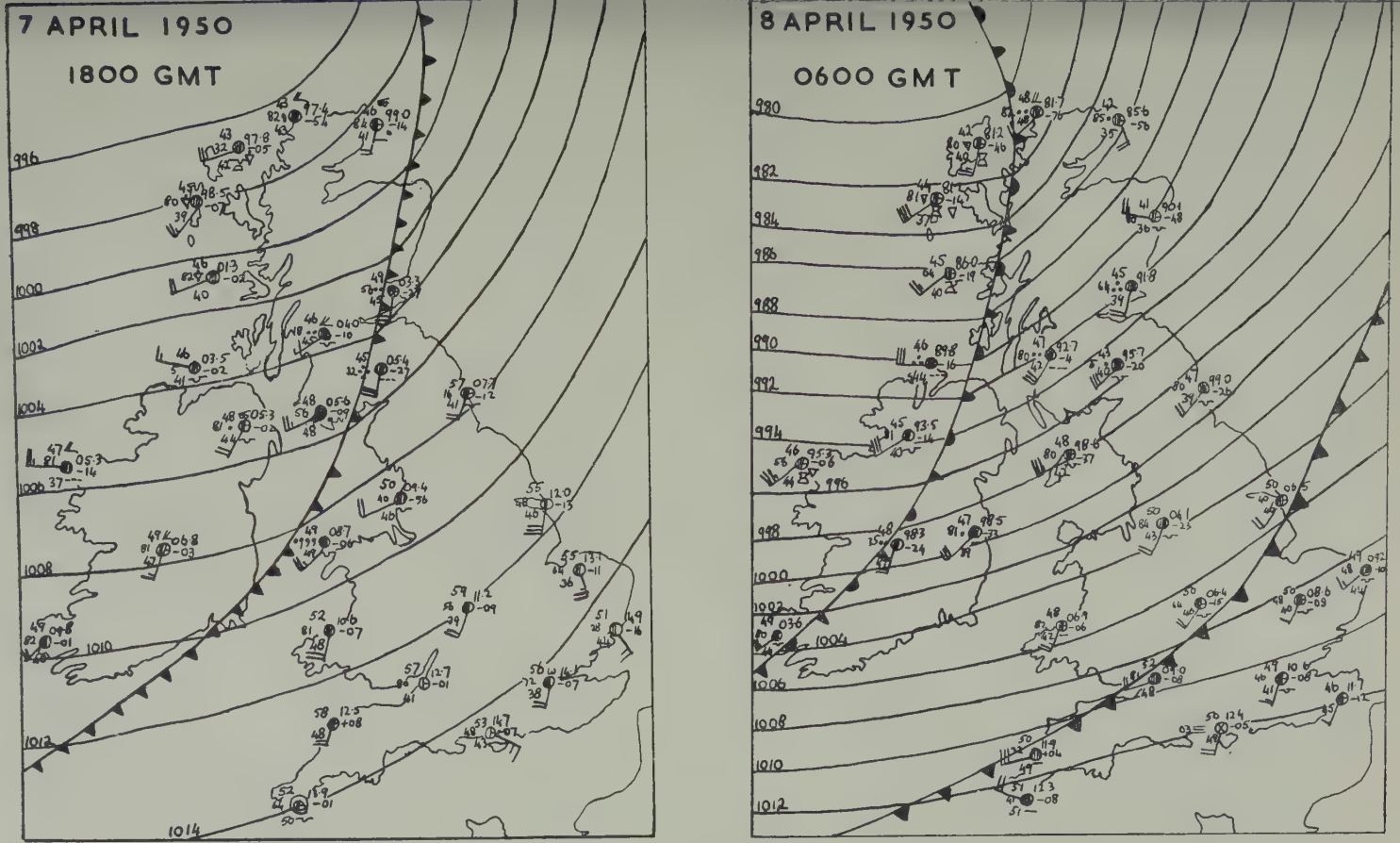
From the earlier work of this investigation, it seems most likely that there is a transition from anafront to katafront as the cyclone develops. It has already been observed (under Section 4b) that most of the katafronts were more actively occluding than the anafronts, and it therefore seems probable that the change is associated with the development of the cyclone. The wind structure, which has already been considered, supplies the reason why this should be expected. A feature of the katafront winds at upper levels was a large component across the front, and this generally demands a well developed upper circulation ; consequently the katafront wind structure is more likely to be found in older depressions than in those whose circulation aloft has not had time to develop. Giao (1929) has shown that a deepening system should give rise to ascending motion of the warm air at the cold front (*i.e.* an anafront), and a filling system to subsidence. This also suggests that katafronts occur at a rather later stage than anafronts, and since the deepening or filling of a depression, and the occlusion of its warm sector, are intimately related (Petterssen 1940), one would expect that katafronts would be found in depressions which are more occluded than those with anafronts.

A test of this inference was made by measuring the width of the warm sector at the point of examination of the front. Since both types were found with equal frequency at the same distance from the centre of the cyclone (Tables VII and VIII), this measurement will give a fairly good indication of the degree of occlusion of the depression. The result, shown in Table IX, clearly indicates that anafronts are always observed before much occlusion has taken place.

TABLE IX. Width of the warm sector at the point of examination of the cold front

	0 – 200 mi	200 – 500 mi	over 500 mi
Anafronts	0	1	10
Katafronts	8	4	9

Initially the cold front is generally an anafront over most of its length, although where it penetrates a strongly anticyclonic region it may appear as a katafront. Usually, however, the katafront occurs first near the centre of an occluding cyclone and spreads outwards along the whole front until it is a katafront throughout.



(a) (b)

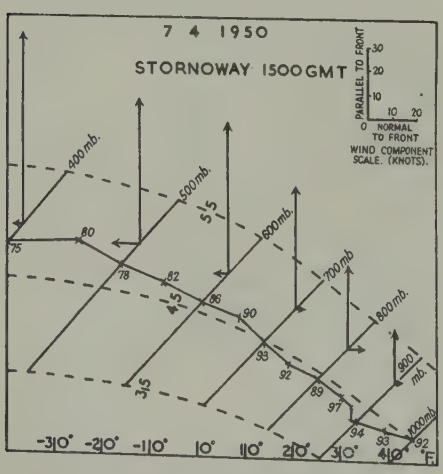
Figure 5. Surface charts for 7 – 8 April 1950, showing transition of cold front from anafront to katafront

As the front weakens with age the amount of subsidence will decrease until finally, in the ultimate quasi-stationary stage, the front may again be a weak anafront, and the cycle will be repeated if further development occurs.

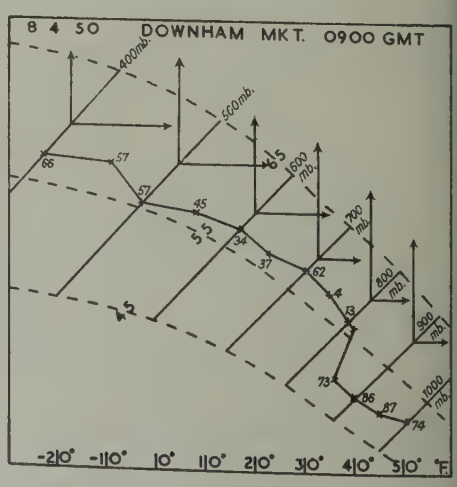
The absence of anafronts during the summer months must mean that the depressions of summer normally reach this country in a later stage of development than in winter, and that the change from anafront to katafront has usually occurred to the west of the British Isles.

A good example of this transition is given by the cold front of 7 - 8 April 1950. Between 1800 GMT on April 7 and 0600 GMT on April 8 this front had completely changed its character from anafront to katafront ; both the temperature sounding and the wind profile show the change clearly. Surface maps for this front are shown in Fig. 5 (a, b) from which it can be seen that precipitation behind the cold front had apparently ceased by 0600 on April 8. The slight deepening of the depression which has occurred is probably the result of the active occlusion seen on the later chart.

The upper air sounding (Fig. 6a) from Stornoway (which was just behind the front) at 1500 GMT on April 7 is a typical anafront sounding. Above 750 mb the component of the wind normal to the front decreased markedly, and remained well below the speed of the front. The sounding (Fig. 6b) from Downham Market on the following morning (0900 GMT) is typical of a katafront, with an inversion at 850 mb and dry air aloft. The upper winds had a considerable component normal to the front, well in excess of the speed of the front, and the direction of the upper winds was at about 40° or 50° to the front instead of 10° to 20° as in the earlier sounding.



(a) 7 Apr. 1950, 1500 GMT.



(b) 8 Apr. 1950, 0900 GMT.

Figure 6. Tephigrams and winds for situation 7 – 8 April 1950.

The wind components normal and parallel to the front are plotted for each 100 mb ; the component normal to the front is plotted relative to the speed of the front, and lies towards the right when positive and to the left when negative. The mean speed of the front at both times was 15 kts.

7. The influence of various factors

(a) *The speed of the front*

It is frequently stated in textbooks that fast-moving cold fronts are generally katafronts, whereas slow-moving or quasi-stationary fronts are anafronts. There appears to be no ground for this statement, as the average speed for anafronts is slightly higher than that for katafronts. The actual mean speeds for all the cold fronts examined are : anafronts 20·5 kt ; katafronts 17·5 kt ; unclassified 16 kt. If the summer fronts, which are generally slow-moving katafronts, are omitted the average speeds become katafronts 21·5 kt, anafronts 20·5 kt. The difference is far too small for any generalization to be made, and it can safely be said that there is no direct relation between the speed of the front and its general behaviour.

(b) *The curvature of the isobars behind the front*

It has been suggested by Petterssen (1940) that when the isobars in the rear of a cold front are curved cyclonically the front will be a fast-moving katafront, and when they are curved anticyclonically it will be a slow-moving anafront. Again, however, there seems to be no evidence to support this view, and, in fact, anticyclonic curvature was found much more frequently behind katafronts, as is evident from Table X.

TABLE X. The curvature of the isobars behind the cold front.

	Cyclonic	Straight	Anticyclonic
Anafronts	7	3	5
Katafronts	7	3	13

Although this test showed that the actual curvature of the isobars was not of very great importance, it seemed desirable to examine further the way in which the curvature was changing in the neighbourhood of the front. Boyden (1938) suggested that this might be an important factor determining surface convergence or divergence at a front, and he assumed that cyclonic curvature increasing towards the front implied convergence, and cyclonic curvature decreasing towards the front implied divergence. Since a study of the winds behind the front has shown that the cold air is generally rising behind an anafront and subsiding behind a katafront, there would be increasing cyclonic curvature towards the front in the former case and decreasing in the latter. There is a further reason why this distribution of curvature would be expected ; the line of a cold front at the surface is a region of disturbed weather conditions indicated by a sharp bend of cyclonic curvature in the isobars. At a katafront the frontal weather is confined to a very narrow region, whereas at an anafront it is generally a fairly broad belt. It is therefore reasonable to expect that the cyclonic bend of curvature will be spread out behind the anafront (*i.e.* there will be increasing cyclonic curvature towards the front in the cold air), but concentrated at a katafront.

Although there were many cases in which it was far from easy to determine the change, if any, in the curvature of the isobars, the results, which are shown in Table XI, are quite striking and are in full agreement with expectation.

TABLE XI. The change in curvature of the isobars in the cold air towards the front.

	+	0	−
Anafronts	11	2	2
Katafronts	0	6	17

\+ Increasing cyclonic or decreasing anticyclonic curvature.
0 No apparent change.
− Decreasing cyclonic or increasing anticyclonic curvature.

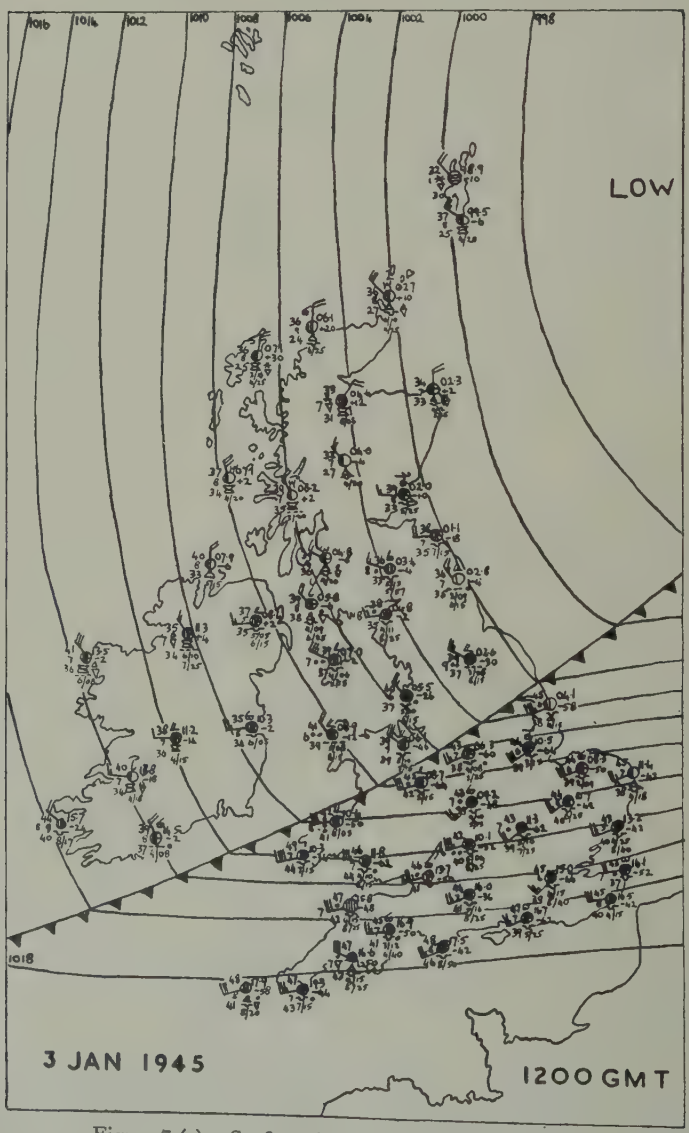


Figure 7 (a). Surface chart for anafront, 3 Jan. 1945

8. Examples

This paper has shown that the anafront and katafront differ in many important respects, and the author suggests that all cold fronts should, whenever possible, be clearly labelled as either anafront or katafront. Anafronts should, of course, always be watched closely for signs of transition into katafronts. It is hoped that the self-explanatory names which have been adopted here will eventually become part of the normal terminology of the synoptic meteorologist.

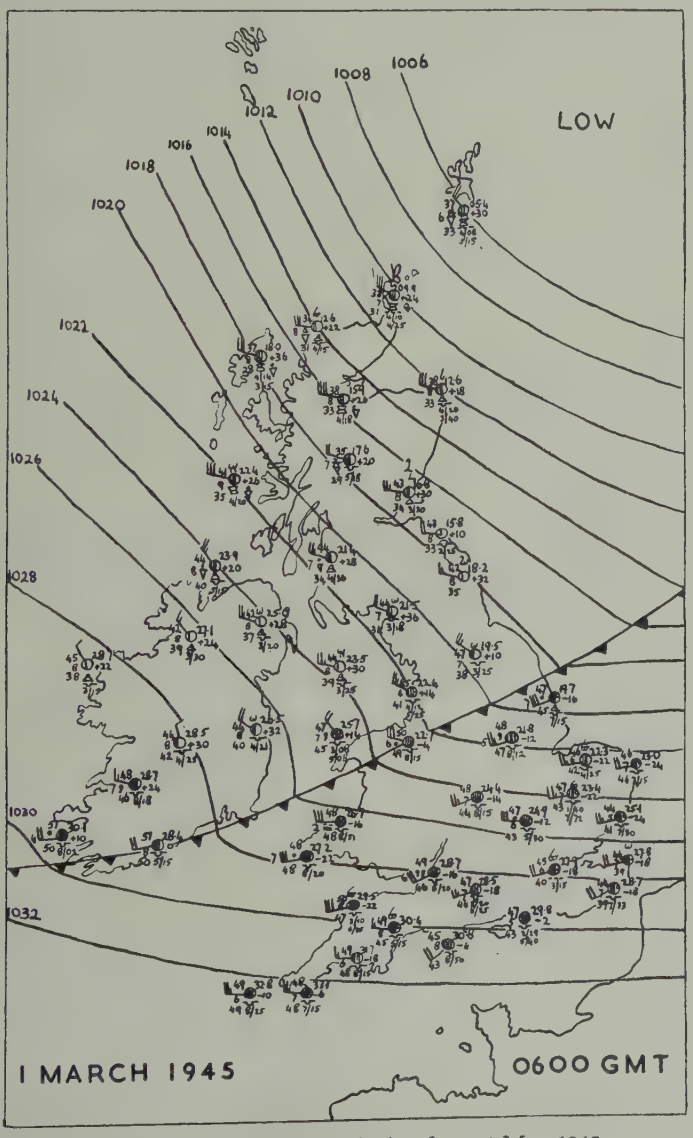


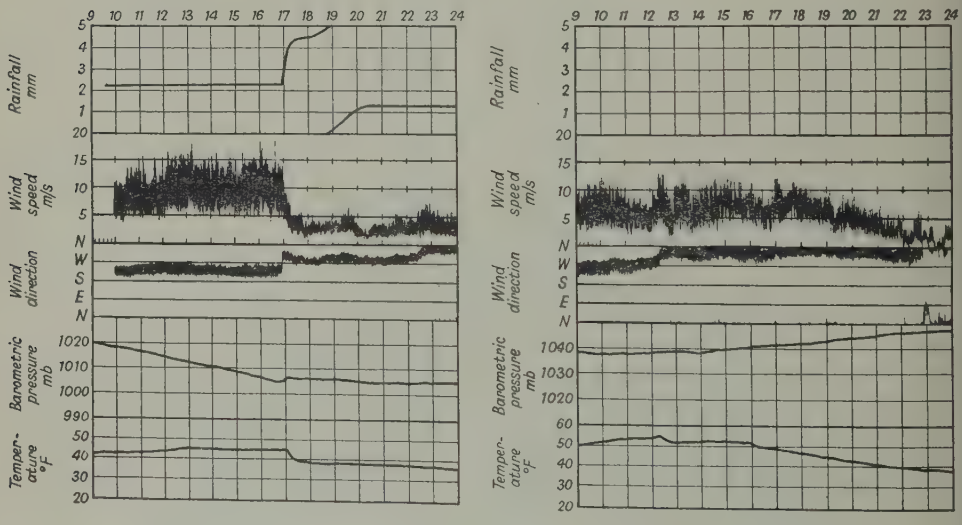
Figure 7 (b). Surace chart for katafront, 1 Mar. 1945.

In order to complete the picture presented so far, the cold fronts of 3 January 1945 and 1 March 1945 have been chosen as examples of the typical anafront and katafront respectively. Surface maps, autographic records, temperature and wind soundings, 700 mb and 500 mb contour charts, and vertical cross-sections for the two situations are reproduced side-by-side in order that the various distinguishing features between the two types can be compared and contrasted.

(a) 3 *January* 1945. (*Anafront*)

(i) *Surface chart, Fig. 7a.* Note the fairly uniform post-frontal precipitation and cloud cover. The warm-sector isobars make a small angle with the cold front, and there is a marked decrease in geostrophic wind-speed at the front. The cold-air isobars have cyclonic curvature increasing towards the front.

(ii) *Autographic Records, Fig. 8a.* These depict the passage of the cold front through Kew Observatory. There is continuous rainfall at the front lasting for some hours. The very sharp wind veer is accompanied by a marked decrease in speed. There is a quite considerable drop in temperature, and a sudden check to the pressure fall. The discontinuities occurred simultaneously on all four charts.

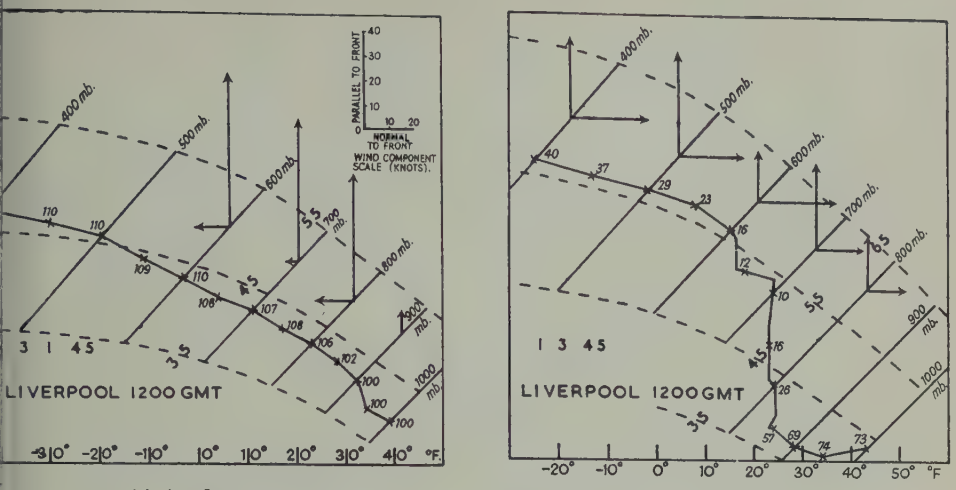

9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24
Rainfall mm
Wind speed m/s
Wind direction
N W S E N
Barometric pressure mb
1020 1010 1000 990
Temperature °F
50 40 30 20
9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24
Rainfall mm
Wind speed m/s
Wind direction
N W S E N
Barometric pressure mb
1040 1030 1020
Temperature °F
60 50 40 30 20

(a) anafront, 3 Jan. 1945

(b) katafront, 1 Mar. 1945

Figure 8. Autographic records showing passage of front through Kew Observatory.

(iii) *Temperature and wind soundings, Fig. 9a.* Note the absence of any sharp inversion and the high relative humidities at all levels. There is only a small angle between the upper winds and the front, and the component of the wind normal to the front decreases with height and is less than the actual speed of the front.



(a) Anafront, 3 Jan. 1945 (b) Katafront, 1 Mar. 1945.

Figure 9. Tephigrams giving upper air temperatures, relative humidity and wind. The wind components normal and parallel to the front are plotted for each 100 mb, when available. The component normal to the front is plotted relative to the speed of the front, and lies to the right when positive, and to the left when negative. Speed of front in each instance = 23 kt.

(iv) *Upper contour charts, Fig.* 10*a, c.* Note the small angle between the contours and the line of the surface front. The wind-speed in the warm air is sub-geostrophic. The isotherms are almost parallel to the cold front. An upper trough is developing, and 6 hr later this had become quite sharp.

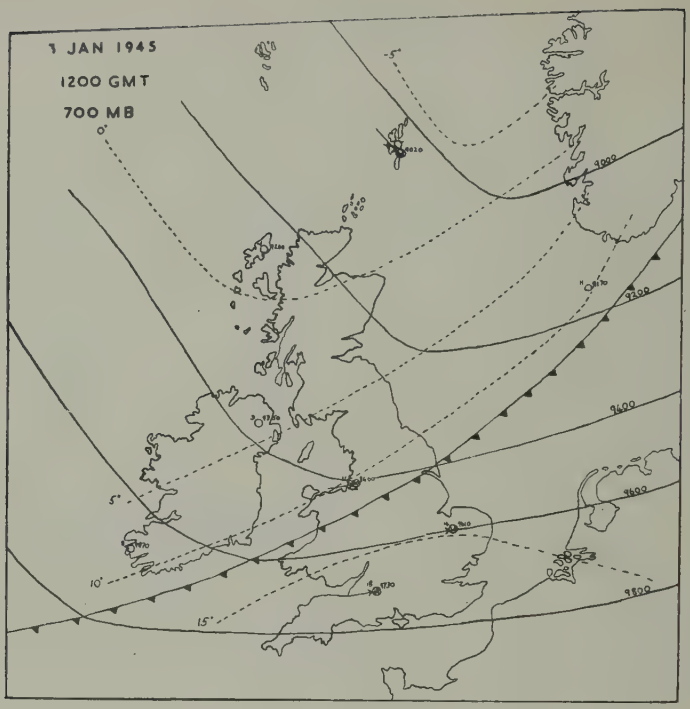
(v) *Vertical cross-section, Fig.* 11*a.* The frontal slope is fairly steep, and the only indications of the frontal zone is a slight refraction of the isotherms and a crowding of the wet-bulb potential temperature lines.

(b) 1 *March* 1945. (*Katafront*)

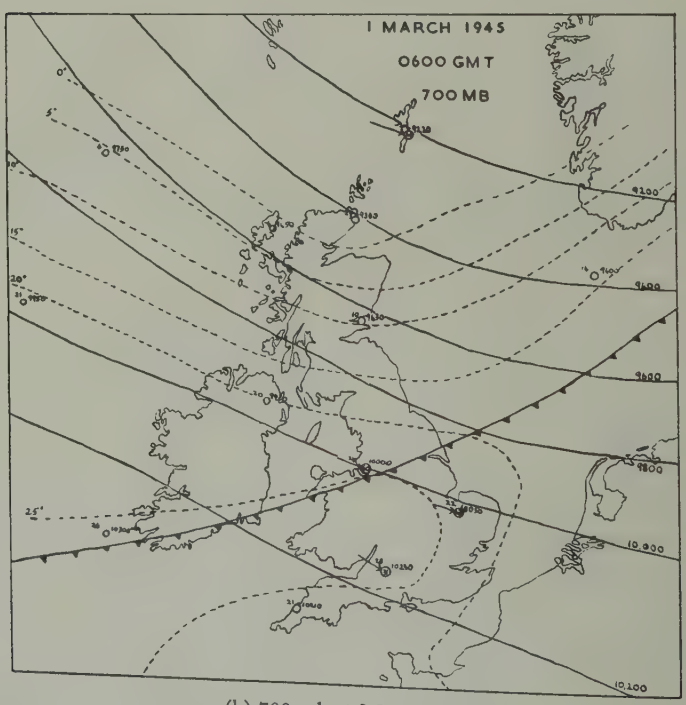
(i) *Surface chart, Fig.* 7*b.* There is a general absence of precipitation behind the front, and a fairly rapid clearance of cloud. The isobars in the cold air have cyclonic curvature decreasing (or becoming anticyclonic) towards the front.

(ii) *Autographic records, Fig.* 8*b.* There was no precipitation at the front at Kew Observatory. The wind veer, although quite sharp, is very slight, and there is no appreciable change in wind speed. There is a fairly sharp temperature fall (sharper than at most katafronts) which was accompanied by a considerable decrease in relative humidity (not reproduced here). The pressure began to rise without any discontinuity.

(iii) *Temperature and wind sounding, Fig.* 9*b.* Note the inversion of temperature and the very dry air aloft. The upper winds are inclined at a large angle to the front, and the normal component increases with height, exceeding the speed of the front.

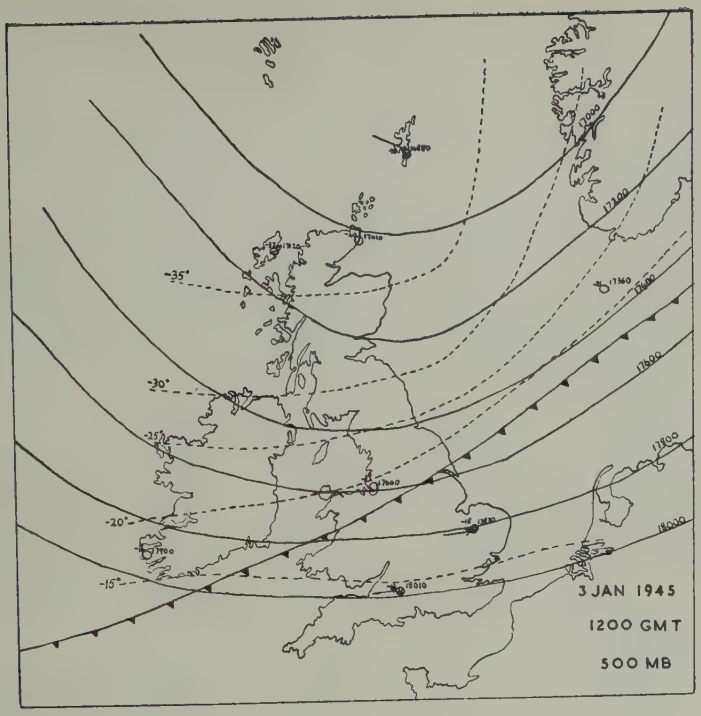


(a) 700 mb, 3 Jan. 1945.

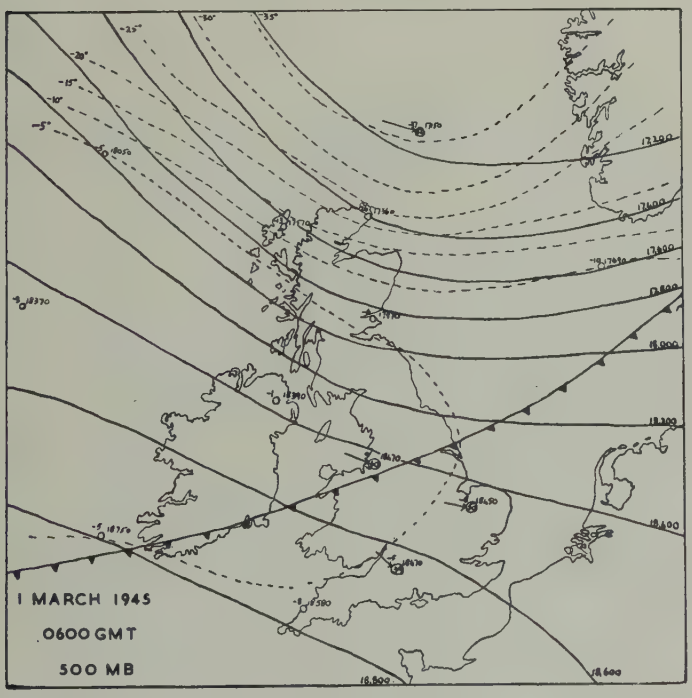


(b) 700 mb, 1 Mar. 1945.

Figure 10 (a, b). Upper contour charts, showing contours and isotherms of the 700 mb pressure surface

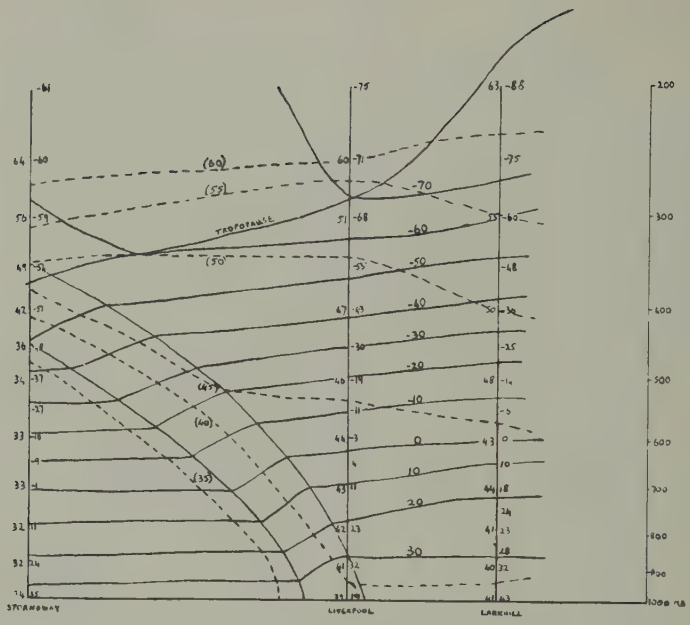


(c) 500 mb, 3 Jan. 1945.

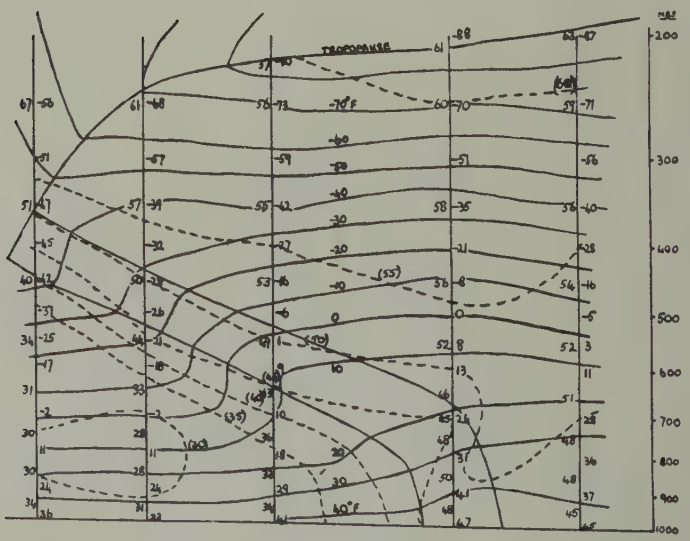


(d) 500 mb, 1 Mar. 1945.

Figure 10 (c, d). Upper contour charts, showing contours and isotherms of the 500 mb pressure surface



(a) Anafront, 3 Jan. 1945.



(b) Katafront, 1 Mar. 1945.

Figure 11. Vertical cross-sections showing wet-bulb potential temperature (broken lines) and dry-bulb temperature (full lines).

(iv) *Upper contour charts, Fig. 10b, d.* The upper winds and contours are inclined at a considerable angle to the front, and the isotherms are directed slightly across the front. There is no sign of an upper trough.

(v) *Vertical cross-section, Fig. 11b.* The frontal slope is much less than that of the anafront, except below 700 mb where it becomes quite steep. In this region the wet-bulb potential temperature lines indicate that there is a zone of convective instability. The frontal zone is clearly shown by a large temperature dicontinuity, and by the crowding of the wet-bulb potential temperature lines, but the base of the temperature inversion clearly lies within the cold air.

Acknowledgments

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Appendix

List of cold fronts on which the investigation was based, with the Upper Air soundings used for the initial classification.

Abbreviations for upper air stations :

D.M. Downham Market
Li. Liverpool
La. Larkhill
Al. Aldergrove
V. Valentia
C. Cornwall (*i.e.* Penzance, St. Mawgan or Camborne)

Type of cold front : A = Anafront. K = Katafront. — = Unclassified.

Under date is given day of month followed by number of month.

The station ahead of the cold front is listed first.

Date	GMT.	Stations	Type
1944			
19/1	18	D.M. & C.	A
23/1	00	La. & Li.	A
19/3	06	La. & Li.	K
7/7	06	D.M. & La.	—
15/7	06	La. & Li.	K
12/8	00	D.M. & Li.	K
25/8	06	La. & C.	K
23/9	00	D.M. & C.	K
11/10	18	D.M. & Li.	K
17/10	12	C. & V.	K
3/11	12	D.M. & Li.	K
5/11	12	D.M. & Li.	A
1945			
3/1	12	La. & Li.	A
10/2	06	D.M. & La.	—
13/2	06	D.M. & Li.	A
23/2	12	La. & Li.	—
1/3	12	La. & Li.	K
25/3	06	D.M. & Li.	A
21/8	06	La. & C.	K
24/10	12	D.M. & La.	—
28/10	06	La. & C.	—
21/11	06	La. & C.	—
2/12	12	D.M. & La.	A
1946			
26/1	18	D.M. & Li.	A
29/1	00	La. & Li.	—
1946			
8/2	18	La. & Li.	A
10/2	18	La. & Li.	K
22/3	06	La. & Li.	A
17/4	06	La. & Li.	—
23/4	18	D.M. & Li.	—
5/6	12	D.M. & Li.	K
28/6	06	D.M. & Li.	K
30/6	06	La. & Li.	K
3/7	06	D.M. & Li.	K
4/7	12	D.M. & Li.	K
25/8	00	D.M. & Li.	K
22/9	18	Li. & Al.	K
1947			
14/3	12	C. & La.	A
24/3	12	D.M. & La.	—
17/4	06	D.M. & Li.	A
13/10	06	La. & Al.	K
2/11	18	D.M. & La.	K
10/11	00	C. & V.	K
23/11	12	C. & V.	K
1948			
5/1	03–09	D.M. & D.M.	A
14/4	15	C. & D.M.	K
12/5	03	D.M. & C.	—
22/5	21	La. & D.M.	A
20/9	03	C. & Li.	—
25/10	09–15	La. & La.	A

The lag of temperature behind solar radiation

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Communicated by Prof. Sir David Brunt, F.R.S.
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SUMMARY

The annual march of air and sea surface temperatures has been examined by harmonic analysis nd characterised by mean and amplitude and first harmonic value of phase. The phase has been xpressed as the lag of temperature behind solar radiation and the data so obtained have been trans- erred to two world maps. A more detailed map relating to air temperatures over the United States also given.

Coastal stations are late in phase and inland basins are earliest. Western margins of the continents how a substantially later phase than eastern margins. This is associated with a tendency towards a ate phase in the sea surface temperatures of the eastern sections of the oceans.

1. INTRODUCTION

For the purpose of climatological studies the annual march of temperature, normally expressed in terms of mean monthly temperatures, can be conveniently epresented by the three characteristics : mean, amplitude and phase. These three re adequate for most extra-tropical localities but it may be convenient at times o include a fourth parameter indicative of the degree of symmetry of the tem- perature curve.

In normal practice it has been customary to deal with annual means, the ange between the highest and lowest mean monthly temperatures, and to use the anuary and July means for air temperatures and the February and August means s representative of sea surface temperatures for the warmest or coldest months. The choice of these months implies a lag between temperature and solar radiation f 25 days for air temperature and 55 days for sea temperature. It is, however, possible to calculate this lag more precisely by means of elementary Fourier analysis nd it is the purpose of this communication to place on record, in the form of maps, he results of the analysis of the available temperature records.

It is hoped in due course to publish in tabular form the temperature charac- eristics for all the stations covered in the present study.

The combination of the three characteristics of mean, amplitude and phase as already proved a very useful tool in comparative climatology in work from these aboratories. It is possible in this way to seek localities which have similar tem- perature régimes throughout the year. The difference in phase between temperature nd evaporation has also proved to be of interest (Prescott 1943).

2. METHOD OF CALCULATION

For the purposes of such an examination it was found adequate to use the irst harmonic of the Fourier series calculated to three terms by the method recom- nended by Whittaker and Robinson (1940).

$$u = a_0 + a_1 \cos x + a_2 \cos 2x + a_3 \cos 3x + b_1 \sin x + b_2 \sin 2x + b_3 \sin 3x$$

$$\text{or } u = a_0 + c_1 \sin (x + \phi_1)$$

where u is the temperature of any month, a_0 is the mean annual temperature, x i the period expressed in degrees where 1 year $= 2\pi$, c_1 is the amplitude of the firs harmonic, and ϕ_1 is the phase of the first harmonic. In the calculations the mea temperature for January is associated with the period value $x = 0^0$; that fo February with $x = 30^0$ and so on.

In the arithmetical procedure employed the values a_3 and b_3 are required t evaluate a_1 and b_1. The value b_2 can be used to obtain an estimate of the degre of symmetry of the curve. When $b_2 = 0$ the curve is symmetrical.

In a similar way the harmonic characteristics of solar radiation are calculate for different latitudes from Angot's tables as quoted by Brunt (1934). Betwee the tropic and the polar circle in the northern hemisphere the angle of phase o the first term ranges from 297·4° to 296·5° and in the southern hemisphere from 115·2° to 115·8°. The change-over between the two hemispheres when the amplitud and angle of phase are both zero occurs at 3·4° N. This variation of phase of sola radiation with latitude is illustrated in Fig. 1.

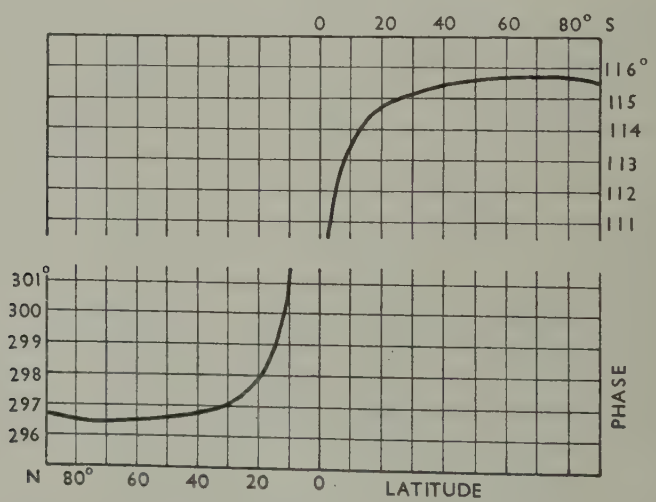


Figure 1. Angle of phase of the first harmonic of solar radiation at north and south latitudes.

In all the calculations the phase of temperature is expressed relative to the phase of this first harmonic of solar radiation. This difference in degrees is trans- formed to days and expressed as the lag of temperature behind solar radiation. Thus for example at the Waite Institute, Adelaide, 35°S. latitude, the angle o phase of the first harmonic for solar radiation is 115·4° and for temperature 81·9° The difference of 33·5° corresponds to 34·0 days.

This difference in phase is essentially the mean difference between the two curves. It may on occasion be desired to compare temperatures more closely, in

hich case use may be made of the second and third harmonics in plotting curves temperature and radiation so that comparisons may be made of the lag at different asons of the year. So far no attempt has been made to develop an arithmetic ocedure for this. Thus for Oxford, England, with a mean lag based on the st harmonic of 32·5°, when all three harmonics are taken into account the lags e as follows :

at minimum values,	winter	25°
at maximum values,	summer	34°
at mean values	spring	36°
	autumn	26°
Mean		30·3°

is probable that in general the error in the estimate of lag due to the neglect of ie second and third harmonics for the most accurate data available is not more ian 2 days.

Within the tropics the sun is overhead twice each year and the estimate of ie difference in phase between air temperature and solar radiation can be made raphically if necessary. The incidence of rainy seasons and the low amplitude o not justify a great deal of attention, however, and where the lag could not readily e determined by arithmetical procedures it has been considered wise not to make ny estimate.

In addition to air temperatures over the oceans, the characteristics of the sea urface temperatures were also determined.

3. Sources of information

In calculating the characteristics of the localities concerned, use was made of tandard texts of climatology, the *Réseau Mondial*, official records generally and ecent climatological summaries, particularly from the United States, Canada, Argentina, France and Australia.

For the oceans, use was made of the *British Meteorological Charts of the Oceans* nd of the *United States Pilot Charts* together with the data published in the *Marine Observer* from 1926 to 1933.

4. Discussion

The temperature characteristics for some typical localities showing extreme alues with respect to lag are given in Table I, and the results in general are sum-narised in three maps (Figs. 2, 3 and 4), two of which are world maps dealing espectively with air and sea surface temperatures and the third giving in greater etail the pattern over a continental area, namely the United States. Maps of Australia and the Mediterranean region have already been published (Prescott 942 ; Prescott and Lane-Poole 1947).

As is to be expected the lag of temperature behind solar radiation is an index f continentality. Coastal stations are late in phase and continental stations relatively arly. Inland basins, in particular in California and the Tarim basin, have the mallest lags, about 20 days, apart from the low values associated with highlands pproaching the tropics. The stations of latest phase are associated with the western

TABLE I. TEMPERATURE CHARACTERISTICS OF LOCALITIES ON THE MARGINS OF CONTINENTS, AND IN CONTINENTAL BASINS AND HIGHLANDS

Locality	Latitude	Longitude	Temperature characteristics		
			Mean °F	Amplitude °F	Phase* Days
Western margins of continents					
Newport, Oregon, U.S.A. ..	44·6° N.	124·0° W.	50·7	7·6	57
Santa Barbara, Cal. U.S.A. ..	34·7° N.	120·0° W.	59·8	6·3	51
Callao, Peru	12·0° S.	77·2° W.	66·6	4·1	65
Lima. Peru	12·0° S.	77·0° W.	66·2	5·8	45
Antofagasta. Chile	23·7° S.	70·5° W.	65·3	5·4	52
Agadir, Morocco	30·5° N.	9·5° W.	65·2	7·8	58
Beirut, Syria	33·9° N.	35·5° E.	68·8	12·5	50
Walvis Bay, S.W. Africa ..	22·7° S.	14·4° E.	61·9	4·6	64
Port Nolloth, S. Africa	29·2° S.	16·9° E.	57·5	2·9	46
Cape Leeuwin, W. Aust. ..	34·2° S.	115·0° E.	62·2	6·1	51
Eastern margins of continents					
St. John's, Newfoundland ..	47·6° N.	52·9° W.	40·1	18·6	46
Miami, Fla., U.S.A.	25·7° N.	81·7° W.	75·1	7·6	42
Rio Grande do Sul, Brazil ..	31·8° S.	52·1° W.	64·4	9·1	44
Amoy, China	24·5° N.	118·1° E.	71·5	13·9	48
Gabo Island, Australia	37·6° S.	149·9° E.	59·1	6·9	45
Tamatave, Madagascar	18·0° S.	49·2° E.	74·5	5·6	38
Continental basins and highlands					
Bagdad, Cal. U.S.A.	35·1° N.	117·3° W.	72·3	20·6	21
Jujuy, Argentina (altitude 4170 ft)	24·2° S.	65·3° W.	63·6	9·7	14
Kashgar, Sinkiang (do 4260 ft)	39·3° N.	76·1° E.	54·6	28·2	20
Pretoria, S. Africa (do 4460 ft)	25·8° S.	28·8° E.	63·5	9·8	12

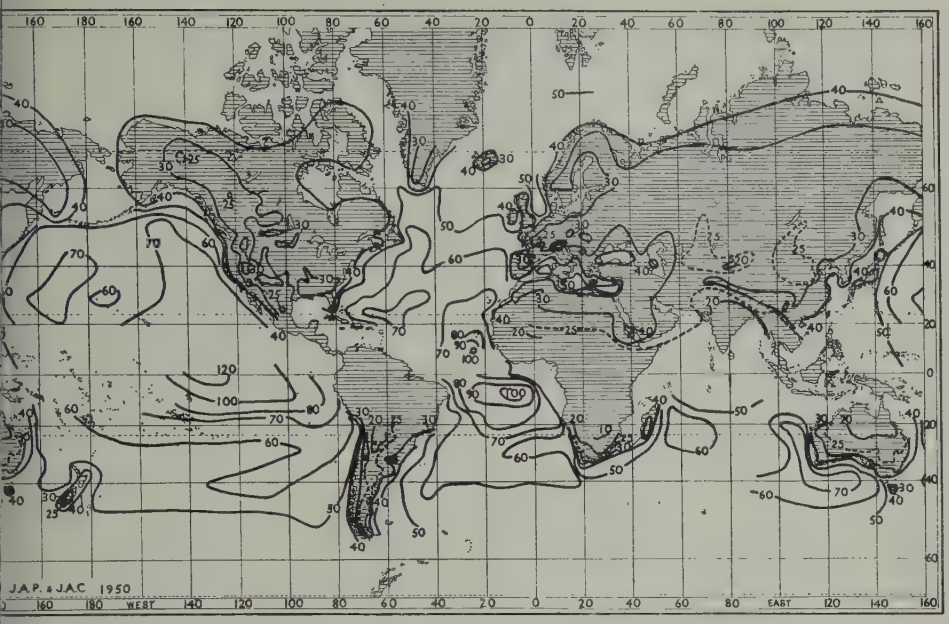
* Lag of temperature behind solar radiation.

margins of the continents and there is in general a substantial difference in this respect between the western and eastern margins.

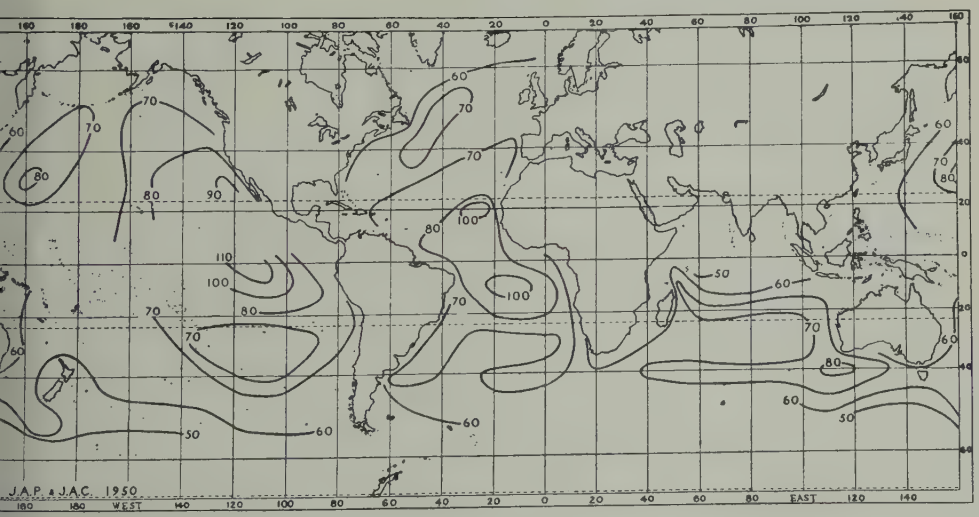
Outside the tropics considerable areas with a lag of 25 days or less occur in the southern hemisphere in Argentina, South Africa and Australia. The plateau country of South Africa is noted for an area with a lag of 15 days or less. In the northern hemisphere apart from a small area in western Europe the main areas with 25 days or less are in Central Asia.

Large seas and lakes have an important influence on temperature phase of adjacent land areas. This is noticeable particularly in the influence of the Mediterranean Sea, the Black Sea, the Caspian Sea and the Red Sea as well as of the Great Lakes of North America.

With respect to sea surface and air temperatures over the oceans it was found that in general the phase of sea temperatures was ten days later than that of the corresponding air temperatures. The greatest lag in sea temperature is rather over 100 days between the equator and the tropic of Capricorn in both the eastern Pacific and eastern Atlantic Oceans. The influence of this late phase on the air temperatures of the adjacent land masses is of some considerable importance. The influence of winds and ocean currents in determining these conditions will be readily recognised.



igure 2. World map on Mercator's projection showing the lag in days of air temperature behind solar radiation.



igure 3. World map on Mercator's projection showing the lag in days of sea surface temperature behind solar radiation.

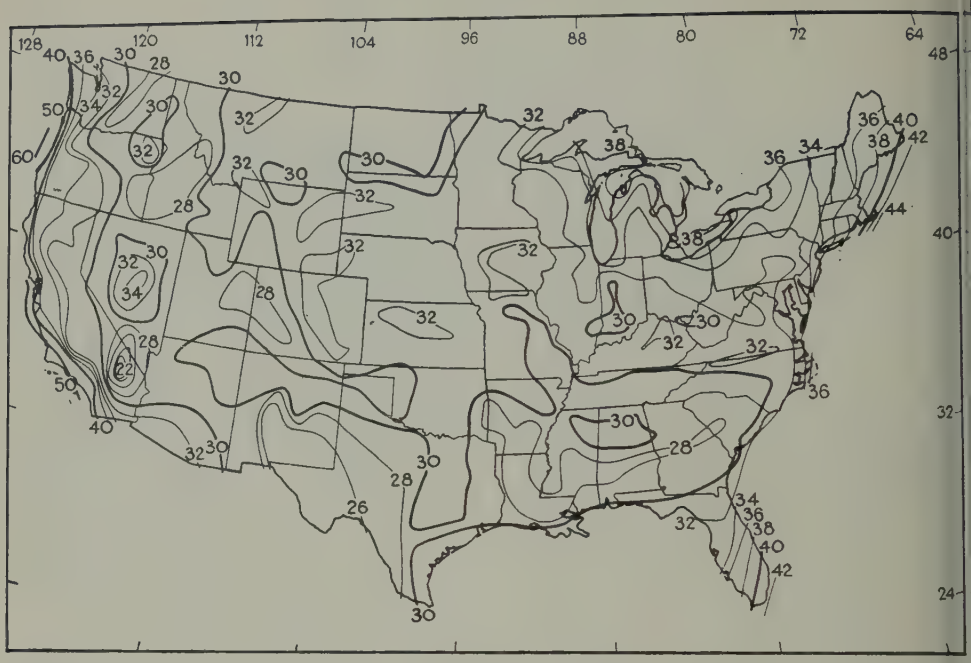


Figure 4. Map of the United States, showing in greater detail the lag in days of air temperature behind solar radiation.

Acknowledgment

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551.521.11(41/2)

An analysis of sunshine values in the British Isles for the period 1909-1948

By D. S. HANCOCK

(Manuscript received 17 July 1950, in revised form 20 September 1950)

1.

The Tables I - V which follow are a continuation of a previous paper by the thor (Hancock 1935) in which an analysis was made of sunshine in the British es for the 25-year period, 1909-33. The series now covers a forty-year period. he values have been prepared in the same manner as in the earlier paper, from e maps published in the Monthly Weather Report (Meteorological Office), by king 40 points uniformly distributed over the British Isles. Maximum and inimum values are shown in the tables in bold face and italics respectively ; here no maximum or minimum appears this occurred in the earlier period, 1909-33.

TABLE I. VALUES OF SUNSHINE DURATION AS PERCENTAGES OF THE MEAN FOR 1909-1933.

ENGLAND and WALES

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Year
..	120	**140**	98	84	105	101	140	105	110	76	58	64	102
..	101	78	102	92	116	95	138	115	94	87	95	115	102
..	85	105	*60*	106	96	87	76	105	73	99	82	129	92
..	78	85	91	67	95	91	71	108	103	77	101	77	87
..	95	98	127	117	88	93	81	92	83	102	102	115	99
..	96	101	96	93	100	93	101	105	93	88	88	100	96
..	90	126	106	120	104	111	82	95	95	94	67	90	98
..	**144**	51	100	77	118	**140**	100	105	117	82	100	101	103
..	80	97	94	72	82	106	120	99	77	96	67	91	90
..	80	73	71	143	108	117	98	97	97	82	97	84	95
..	91	120	117	109	122	100	117	88	99	108	102	109	107
..	97	93	98	104	107	115	103	97	97	92	85	95	99
..	83	92	115	97	115	85	*64*	110	96	89	76	117	95
..	112	101	112	126	92	91	99	96	*67*	95	71	98	97
..	107	110	95	124	103	88	107	86	76	80	63	**148**	99
..	113	*50*	63	100	96	92	94	**160**	103	102	123	78	98
..	81	98	146	128	130	86	98	77	87	89	109	141	106
..	99	90	106	115	107	88	92	106	86	91	88	116	99
r/day	2·17	3·31	5·79	7·54	8·54	9·29	9·45	8·65	6·49	4·30	2·91	1·89	4·60
..	1940	1934	1929	1914	1909	1940	1911	1947	1911	1921	1920	1946	1921
r/day	0·95	1·17	2·17	3·03	3·69	4·39	3·63	2·88	3·11	2·19	1·19	0·75	3·35
..	1917	1947	1936	1920	1932	1909	1944	1912	1945	1915	1934	1912	1912
r/day, 48 ..	1·49	2·31	3·62	5·06	6·20	6·54	5·69	5·42	4·50	3·15	1·97	1·30	3·94

TABLE II. VALUES OF SUNSHINE DURATION AS PERCENTAGES OF THE MEAN FOR 1909-1933.
SCOTLAND

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
1934 ..	115	104	110	82	94	114	125	86	112	82	98	54
1935 ..	94	105	107	91	136	84	136	94	92	*64*	94	**160**
1936 ..	104	110	58	116	108	119	88	99	86	115	82	88
1937 ..	83	117	108	*68*	121	88	83	110	103	94	82	109
1938 ..	95	93	83	105	99	89	84	141	92	98	68	121
1934-38 ..	98	106	93	92	112	99	103	106	97	92	85	106
1939 ..	111	78	99	101	99	117	67	110	101	135	*65*	75
1940 ..	98	93	90	75	109	**147**	114	102	100	67	74	100
1941 ..	109	96	103	74	97	105	82	94	81	103	83	108
1942 ..	66	102	81	**147**	127	75	116	68	100	86	101	97
1943 ..	90	93	101	87	106	95	125	77	94	80	82	93
1939-43 ..	95	93	95	97	108	108	101	90	95	94	81	95
1944 ..	*57*	107	91	78	*72*	76	63	118	91	79	97	95
1945 ..	123	98	82	94	98	88	78	122	86	108	89	92
1946 ..	113	129	91	94	**141**	87	99	103	94	103	71	132
1947 ..	107	119	97	78	92	69	86	**196**	91	102	122	74
1948 ..	82	100	125	117	135	93	103	70	91	107	106	138
1944-48 ..	96	111	97	92	108	83	86	122	91	100	97	106
Max. hr/day	1·52	3·29	5·26	6·89	7·77	8·57	6·59	8·22	4·96	3·53	2·40	1·44
Year ..	1931, 1933	1930	1929	1942	1946	1940	1917	1947	1933	1922	1925	1935
Min. hr/day	0·65	1·32	1·86	3·21	3·92	3·01	3·13	2·20	2·91	1·61	1·06	0·32
Year ..	1944	1923	1928	1937	1944	1912	1931	1912	1930	1935	1939	1912
Mean hr/day, 1909-48 ..	1·14	2·08	3·24	4·60	5·62	5·78	4·67	4·29	3·67	2·57	1·60	0·93

TABLE III. VALUES OF SUNSHINE DURATION AS PERCENTAGES OF THE MEAN FOR 1909-1933.
IRELAND

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
1934 ..	120	109	105	92	94	112	134	94	101	87	78	71
1935 ..	74	120	94	99	**137**	82	120	98	88	93	118	124
1936 ..	90	94	*67*	99	117	100	78	96	96	107	92	82
1937 ..	77	108	112	*50*	123	73	78	124	92	98	85	89
1938 ..	85	92	82	**150**	87	89	79	91	86	94	86	**145**
1934-38 ..	89	105	92	98	112	91	98	101	93	96	92	102
1939 ..	89	80	97	108	114	135	63	118	85	**126**	71	84
1940 ..	120	61	85	75	101	**152**	107	97	103	66	107	70
1941 ..	80	125	88	78	105	87	77	101	93	108	113	74
1942 ..	128	87	79	119	114	108	109	73	82	95	100	71
1943 ..	89	75	95	78	112	115	118	82	80	99	58	133
1939-43 ..	101	86	89	92	109	119	95	94	88	99	90	86
1944 ..	72	123	115	96	95	82	*60*	116	95	97	71	96
1945 ..	109	88	88	106	92	90	73	101	75	107	74	87
1946 ..	102	86	95	106	126	96	93	87	*67*	67	106	133
1947 ..	93	76	74	93	101	82	81	**181**	78	108	105	70
1948 ..	109	87	120	104	133	95	97	88	86	85	100	121
1944-48 ..	97	92	98	101	109	89	81	115	80	93	91	101
Max. hr/day	2·36	3·22	6·01	7·75	7·80	8·82	7·97	8·29	5·86	3·58	3·04	1·86
Year ..	1933	1930	1929	1938	1935	1940	1911	1947	1933	1939	1909	1938
Min. hr/day	1·00	1·32	2·44	2·56	3·95	3·66	2·99	3·03	2·76	1·73	1·09	0·89
Year ..	1915	1926, 1929	1936	1937	1925	1923	1944	1932	1946	1916	1917	1931
Mean hr/day, 1909-48 ..	1·50	2·25	3·56	5·12	5·96	5·79	4·89	4·64	3·96	2·83	1·98	1·29

TABLE IV. VALUES OF SUNSHINE DURATION AS PERCENTAGES OF THE MEAN FOR 1909-1933.

BRITISH ISLES

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Year
..	117	125	103	86	99	105	134	98	96	80	75	64	102
..	91	94	100	94	125	88	133	106	84	76	101	125	101
..	90	104	*61*	106	105	98	78	102	72	105	86	106	93
..	78	113	101	*62*	109	82	74	110	89	86	94	86	90
..	92	105	104	124	90	91	81	103	76	99	91	124	98
38 ..	94	108	94	94	106	93	100	104	83	89	89	101	96
..	93	112	102	112	106	119	74	105	84	111	68	85	98
..	127	*70*	93	76	111	**145**	97	103	97	75	97	91	99
..	185	116	94	74	92	101	101	99	73	101	85	89	93
..	90	92	76	137	114	105	101	75	83	87	94	82	95
..	89	112	107	95	116	103	118	84	83	101	86	113	101
43 ..	97	100	94	99	108	115	98	93	84	95	86	92	97
..	75	115	110	92	97	82	*63*	114	84	91	80	106	92
..	112	107	98	115	93	90	88	103	*65*	101	76	93	95
..	105	119	94	114	119	90	96	90	69	82	78	**139**	100
..	105	81	73	93	96	84	92	**174**	83	104	119	75	98
..	89	106	134	116	**132**	90	99	79	78	92	107	134	105
48 ..	97	106	102	106	107	87	88	112	76	94	92	109	98
hr/day	2·01	2·81	5·72	7·03	7·77	8·98	8·21	8·44	5·67	3·73	2·74	1·66	4·39
ar ..	1933	1930	1929	1914	1948	1940	1911	1947	1911	1919	1925	1946	1911
hr/day	1·00	1·44	2·18	3·07	4·34	4·30	3·36	2·76	3·13	2·19	1·23	0·71	3·23
ar ..	1914	1940	1936	1937	1932	1923	1944	1912	1945	1916	1912	1912	1912
n hr/day, 09-48 ..	1·41	2·24	3·51	4·96	5·99	6·15	5·21	4·93	4·14	2·92	1·88	1·23	3·71

2.

Table V shows the years 1909-1948 arranged according to the yearly values (in hours per day) for the British Isles as a whole. In some cases it has been necessary to make slight corrections to the figures appearing in the list of the earlier paper.

TABLE V. YEARS IN ORDER OF AMOUNT OF BRIGHT SUNSHINE FOR BRITISH ISLES.

1911	4·39	1948	3·84	1910	3·72	1936	3·49
1921	4·12	1928	3·83	1942	3·68	1941	3·47
1929	4·12	1925	3·82	1930	3·67	1924	3·46
1933	4·00	1934	3·82	1946	3·65	1931	3·46
1914	3·94	1940	3·82	1926	3·64	1913	3·45
1919	3·94	1943	3·80	1923	3·62	1932	3·43
1915	3·91	1922	3·78	1938	3·61	1916	3·42
1935	3·88	1917	3·75	1927	3·60	1944	3·42
1909	3·86	1939	3·75	1945	3·54	1937	3·38
1918	3·85	1947	3·74	1920	3·49	1912	3·23

3.

The correlation of the percentage departure from the monthly mean with the departures from the means for other months in the *same* year has been evaluated

and the coefficients are given in Tables VI and VII. Values numerically less than 0.5 have not been included.

Table VIII gives the correlation of departures between one part of the British Isles and another for each month of the year.

In order to aid in the assessment of the significance of the correlations shown in Tables VI - VIII the standard errors are given below :

C.C.	0·3	0·4	0·5	0·6	0·7	0·8	0·9
S.E.	0·14	0·13	0·12	0·10	0·08	0·06	0·01

TABLE VI. CORRELATION OF SUNSHINE FOR ONE MONTH WITH THAT FOR OTHER MONTHS IN THE SAME YEAR. PERIOD 1909-48.

Upper right—England and Wales. Lower left—Scotland.

Scotland \ England and Wales	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
J.		.	.	**·59**	**·50**	.	.	·58	.	.	.	.
F.	·62		.	.	.	.	.	.	.	.	.	.
M.	·52	.		.	.	.	.	.	.	.	.	·51
A.	**·55**	**·52**	.		·53	.	.	**·50**	.	.	.	.
M.	.	.	.	.		.	.	.	.	.	.	.
J.	·51	.	.	.	.		·57	.	·64	.	.	.
J.	.	.	.	.	.	.		.	·73	**·85**	.	.
A.	.	.	.	**·60**	.	.	.		·62	.	.	.
S.	.	.	.	.	.	.	·51	.		.	.	.
O.	.	**·52**	.	.	.	.	.	.	.		.	.
N.	.	.	.	.	.	.	·55	.	.	·50		.
D.	.	.	.	.	·53	.	.	.	.	.	.	

Negative correlations are shown in bold face.

TABLE VII. CORRELATION OF SUNSHINE FOR ONE MONTH WITH THAT FOR OTHER MONTHS IN THE SAME YEAR. PERIOD 1909-48.

Upper right—Ireland. Lower left—British Isles.

IRELAND AND THE BRITISH ISLES, 1909-1948 :—

British Isles \ Ireland	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
J.		.	.	.	.	.	.	.	·56	.	.	**·53**
F.	.		.	**·52**	.	.	.	.	.	.	.	.
M.	.	.		.	.	.	.	.	.	.	.	·58
A.	**·57**	.	.		.	.	.	.	.	.	.	.
M.	.	.	.	·89		.	·55	.	.	.	.	.
J.	.	.	.	.	**·57**		.	.	.	**·58**	.	.
J.	.	.	.	.	·58	**·63**		.	·58	**·56**	.	.
A.	.	.	.	.	.	.	.		.	.	·51	.
S.	.	.	·55	.	·66	·59	·71	·55		.	·57	.
O.	.	.	.	.	.	.	.	.	.		.	.
N.	.	.	.	.	.	.	.	.	.	.		.
D.	.	.	·54	.	.	.	.	.	.	.	.	

Negative correlations are shown in bold face.

ABLE VIII. Correlation of sunshine in different parts of the British Isles, 1909-1948.

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
ngland and Wales to Scotland	·83	·35	·76	·83	·70	·83	·71	·87	·82	·54	·79	·81
ngland and Wales to Ireland	·66	·49	·71	·81	·71	·88	·76	·88	·84	·69	·68	·71
cotland to Ireland	·66	·72	·82	·84	·88	·84	·87	·90	·71	·78	·81	·77

All the above figures are positive.

Acknowledgment

The author wishes to acknowledge the kindness of the Director of the Eire Meteorological Service for permission to include values for Southern Ireland, obtained during the war years, to complete the Monthly Weather Report maps.

Reference

Hancock, D. S. 1935 *Quart. J. R. Met. Soc.*, **61**, p. 45.

551.557 : 551.509.67

Wind shear revealed by artificial nucleation

By R. FRITH

Meteorological Office Research Flight, Farnborough

Manuscript received 25 October 1950

1. The experiment

When a supercooled cloud is inoculated with a sufficient number of ice crystals (*e.g.* by seeding with dry ice) the inoculated volume may become completely cloudless (by precipitation of snow) or may change from a supercooled water cloud to an ice-crystal cloud. With a moderate seeding the former is likely to happen ; with a heavy seeding, the latter. In either case that particular volume is marked, and subsequent developments and movements may be watched, photographed and measured.

On 25 January 1950 there was complete cover of supercooled Sc cloud over the greater part of southern England. So far as could be observed there were no ice crystals in the cloud. The cloud top was at about 4,500 ft and the base at about 2,000 ft. The temperature in the clear air just above the cloud varied between 15° and 20°F. The surface pressure gradient was rather flat but the Downham

Market radio-sonde indicated winds at cloud level from about 045°, 10-20 kt. No fronts were shown on the Central Forecast Office charts for this area.

This cloud was seeded on a line about 12 mi long running at right angles to the wind. Fig. 1 shows the approximate location of the seeded lane.

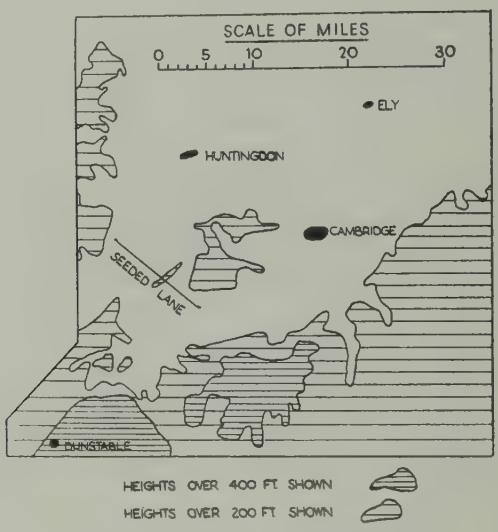


Figure 1. Position of seeded lane.

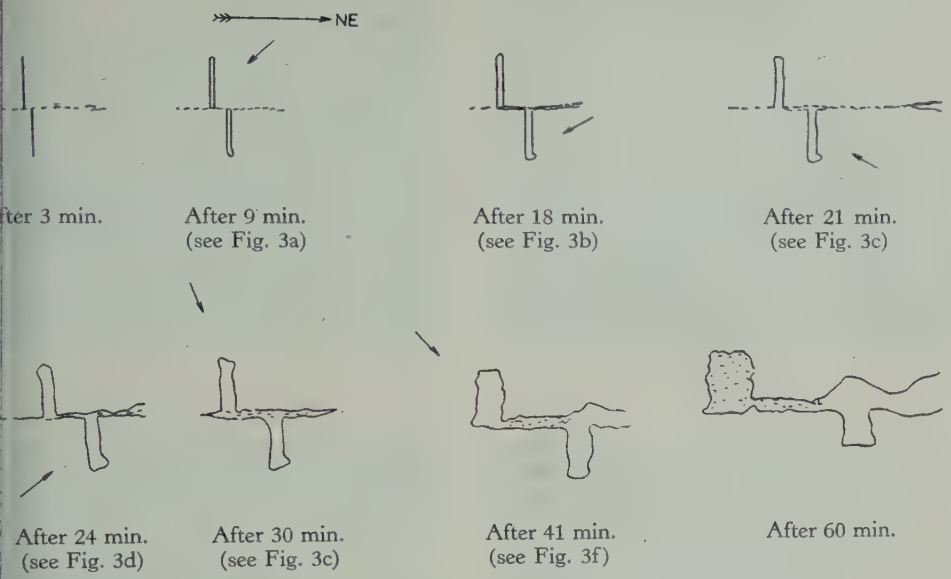
The seeding began at about 14.30 GMT and was carried out from a Halifax flying just above the cloud. Dry ice pellets, of diameter between 0·2 and 1·0 cm, were dropped at the rate of 4 lb./mi. Developments were observed from a Mosquito flying at 15,000 ft. After seeding, the Halifax also climbed to 15,000 ft and took a series of photographs.

At the time of seeding the sheet of cloud was unbroken and uniform except that far to the NE. there appeared to be a clear area ; and running SW. from this clear area was a clearly defined *rift* in the cloud. As the photographs show, half an hour or so after seeding, the cloud was breaking up along this rift but at the time of seeding no holes were visible. It is important to note that the seeded lane was bisected at right angles by the rift.

The developments are represented in Fig. 2, which also indicates the angles from which the six photographs (see Fig. 3) were taken.

2. The developments

The first apparent result of the seeding was the appearance of a dark line on the cloud top, which rose to a level slightly higher than the general cloud top. The dark line is seen in the foreground of Fig. 3 (a) taken 9 min after seeding



igure 2. The development of the seeded area as seen from an aircraft. The rift extended to a ear area to the NE., but faded out about 10 mi to the SW. of the seeded lane. The arrows show the directions from which the photographs were taken.

t this stage it appeared as if the seeding aircraft had altered course at the rift ; ter it became evident that this apparent bend was the first indication of the existence f a strong shear of wind across the rift — the winds being stronger to the north an to the south.

Fig. 3 (b) shows part of the seeded area after 18 min. The dark cloud is ow probably slightly lower than the general cloud top.

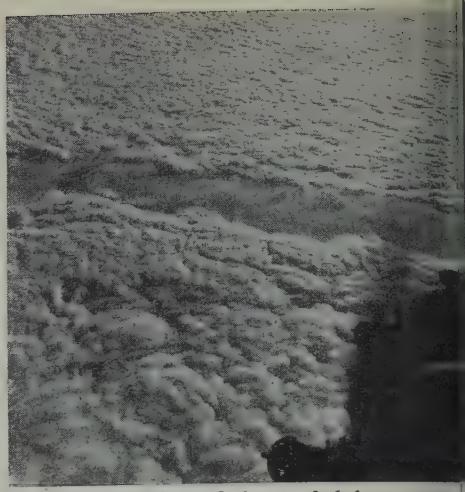
Fig. 3 (c) provides a view of the whole seeded area, taken 21 min after seeding. he infected cloud is divided quite sharply into two almost equal parts, separated long the rift. As will be seen, the separation of these two halves increased pro- ressively with time. The deformation, or head, in the foreground must be due o some secondary shear of wind.

In Fig. 3 (d) we see this deformation even more clearly and also, now, the reaking up of cloud along the rift. Note also the cliff-like edges to the infected area.

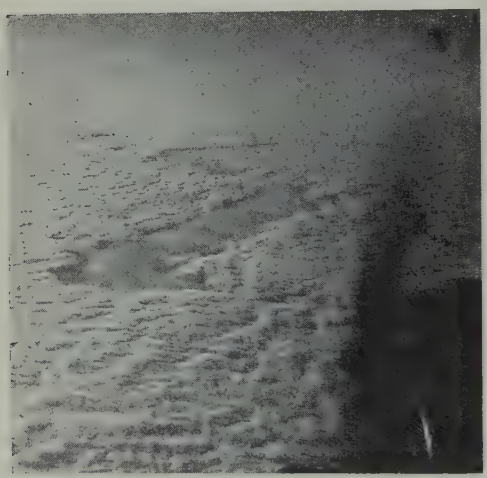
Fig. 3 (e) provides another view of the whole area, 30 min after seeding. At his time the northerly infected area was about 8 mi long and 1 mi wide. The outherly infected area was smaller, and more regular in shape, with sharper edges.

At this time the clouds were being investigated by the Mosquito, flying at bout cloud level. By timing flight from point to point the two sections were ound to be about 5 mi apart — indicating a shear of about 10 mi/hr. Brilliant un images and haloes were observed — confirming that the cloud, both in the seeded reas and in the five-mile-long connecting lane, consisted of ice crystals, (although n the connecting lane there was also a good deal of broken Sc — as may be seen n Fig. 3 (f)).

(a) The first part of the seeded lane, 9 min. after seeding.

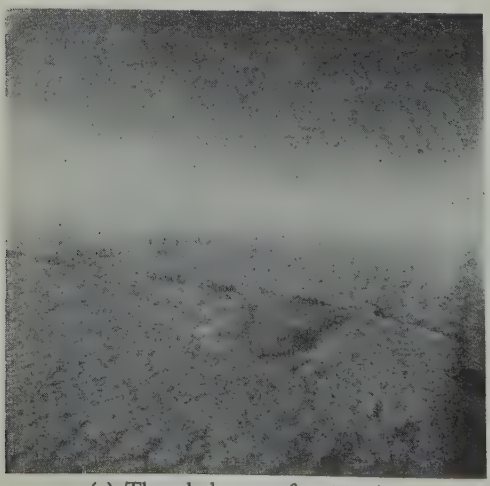


(b) The beginning of the seeded lane, 18 mi after seeding. The peculiar distortion is alrea visible on extreme left.



(c) Whole of the seeded area, 21 min. after seeding, showing the distortion at the start, and the separation into 2 distinct portions along the rift.

(d) The southerly infected area, 24 min. af seeding. Note the cliff-like edges to the stra cumulus.



(e) The whole area after 30 min.

(f) The whole infected area after 41 min. N the irregular outlines of the northerly hole (fo ground). The southerly hole is visible ab 1/3 down centre.

Figure 3.

The photograph shown in Fig. 3 (f) was taken after 41 min. In the foreground is the northerly infected area. This is now beginning to resemble a natural hole and Sc is beginning to reform. The infected part of the rift, now about 10 mi long and 0·5 mi wide, contains much broken Sc. The southerly infected area can also be seen.

About one hour after seeding, the ground was visible through both parts of the seeded area ; but *virga* could be seen still falling from the sharp edges of the southerly area. The two sections were then 15 to 20 mi apart.

No radar returns were observed at East Hill, the Meteorological Office Radar Station near Dunstable.

3. Discussion

The general result of the seeding was as expected. A lane of ice-crystal cloud was produced which spread at about 1 mi/hr, and the ice crystals slowly precipitated.

The separation of the seeded area into two distinct parts, indicating a very strong shear of horizontal wind, was not expected. The shape of the southerly area indicated a further wind shear. Two estimates of the strength of the shear across the rift gave, after 30 min, 10 kt ; and, after 60 min, 15 to 20 kt. The rift line between the two areas became about 1 mi wide but originally it was certainly less than 0·25 mi across. Thus the shear was of the order 15 mi/hr in 0·25 mi or less. It is interesting to note that, in spite of this strong shear, neither aircraft reported bumpiness.

This work probably provides the first evidence that strong horizontal wind shear can exist in the free air.

Acknowledgment

Acknowledgment is due to the Director of the Meteorological Office for permission to publish this note.

Correspondence

Large-scale vortex motion in the atmosphere

If I may be permitted the space I would like to make some observations upon Mr. R. W. James's paper *On the theory of large-scale vortex motion in the atmosphere* (*Q.J.*, **76**, p. 255).

Mr. James has traced the theory of vortex motion using certain parameters. One of the most important parameters is the intensity h, which is "the difference between the pressure, p, at the centre, and that at a very great distance therefrom (p_0)." It is a matter of observation to assign a value to p. On an infinite plane, under ordinary conditions, there is no difficulty in deciding upon a value of p_0 ; *i.e.*, when there is not an infinite number of vortices with density distribution greater than some given number, and there is no agency violently disturbing the general fluid motion. In this case p_0 is the "pressure at infinity" and is independent of the direction from the centre of the vortex. We could construct examples on the infinite plane in which p_0 is not independent of the direction from the axis, but the fields of pressure would not, perhaps, correspond to those that we normally assume to exist. When the surface is not infinite but is closed, as is the earth's surface, there is a fundamental difficulty in determining the point at which p_0 is to be measured. In the case of a single vortex there may be a case for measuring p_0 at the point antipodal to the centre of the vortex. When systems of vortices are in existence it is not at all clear that p_0 exists, and there does not seem to be any rule determining either the distance from the centre or the direction in which to proceed from the centre of any one vortex to specify the point at which p_0 is to be measured. It is less clear as to whether p_0 is meant to be common to all vortices of the system or whether each vortex has an associated value p_0. The root of the trouble is that in moving away from the centre of one vortex we may come into the field of another vortex or into the common field of a subsidiary system of vortices without being able to specify a proper value to p_0.

A second parameter which plays a vital role in Mr. James's paper is the spread radius, R, defined as "the distance from the centre at which the gradient of pressure becomes a maximum." At every level z we can find a distance $R(z)$ from the centre or axis of the vortex at which the pressure gradient at that level is a maximum. R is the maximum value of $R(z)$ and in particular may not be measured at the surface. R is the distance from the axis to the point at which $\rho v/\sin\theta$ is a maximum, where v is the geostrophic wind speed, ρ the density, and θ the latitude. It is a common synoptic experience that the geostrophic momentum does not always have its maximum at the surface. Goldie (1949) gives some characteristic variations of momentum with height, illuminating this point. Yet the values given for R in Table I of Mr. James's paper, following Goldie, are values of R_0 measured at the surface.

A third comment concerns some of the calculations. In section 3 Mr. James finds difficulty with the Rankine vortex because the kinetic energy contains a divergent infinite integral. The integration is carried out over an infinite plane, whereas for atmospheric considerations the integration should be carried out over the closed surface of the earth. The trouble caused by integration over an infinite plane is again apparent in para. 4. For example, consider Eq. (13).

$$P = \frac{c_p}{k}\int (p - p_0)\, d\sigma\, dz = \frac{c_p}{k}\int hf(r/R)\, d\sigma\, dz$$

and to obtain the result of Eq. (13) we must put $d\sigma = 2\pi r\, dr$ and integrate from $r = 0$ to $r = \infty$. We should integrate over the closed surface of the earth. Now this is not the same criticism often levelled at the dynamicians who use co-ordinates in the local tangent plane as an approximation to the true curvilinears. In this case, following the derivation as above, the infinite integral is used to limit the form of the function f, so that the integral over the entire infinite plane is necessary to the argument. Over a closed surface the infinite integral never arises and indeed attention is more necessary to dealing with any poles that may arise in the expressions for the kinetic energy and other calculated properties of the system.

In view of these comments it seems to me that there are grave difficulties in applying Mr. James's theory to atmospheric phenomena.

This letter is submitted by permission of the Director, Meteorological Office.

E. KNIGHTING

Meteorological Office,
London.
1 November 1950

REFERENCE

Goldie, A. H. R. 1949 *Weather*, **4**, p. 345.

REPLY

If a vortex occupies an area small compared with that of the earth's surface, it may be treated as planar to a first approximation, and all the essential physics of the situation is embodied in the plane approximation. If this were not so the study of plane vortices would be pointless, and he would be venturesome indeed who presumed to plot synoptic maps on a flat surface .

The whole point about the Rankine vortex is that its area of influence is diffused over a very wide area, and hence the flat-earth approximation becomes inappropriate. Applying the Rankine vortex to the earth, Mr. Knighting finds no difficulty about infinity, because there is no infinity. But although the Numidian Lion does not touch the sky, he still eats up the villagers. My objection was essentially a physical one, although couched in mathematical terms. If, for example, we have a Rankine vortex centred on the pole and cutting off at the equator, with a spread radius of 500 km, 55 per cent. of the total kinetic energy only is confined within a radius of 1,500 km about the pole, the remaining energy being at a greater distance from the centre of the vortex. The tail may not be wagging the dog, but it is certainly upsetting the canine equilibrium.

I think it will be universally agreed that a vortex is a more intensive organization than this, that the energy is effectively concentrated at no great distance from the centre. If vortices were, in fact, as diffuse as the Rankine model suggests, the forecaster at Dunstable would be perpetually worrying about the backwash from that Andean low. The dog is certainly shaggy, but not *so* shaggy. Happily, we can still treat a few thousand kilometres as infinity for all practical purposes.

The remainder of Mr. Knighting's comments concern the measurability of my suggested parameters in practice. It is agreed that there are difficulties in measuring the " pressure at infinity." We can conceive of p_0 as the pressure at a point in the absence of any perturbation. This implies that it is possible to analyse a field of pressure into a general flow-field, and a perturbation, *i.e.*, the vortex. Mathematically we can split up a field in any way we please ; the difficulty lies in deciding what breakdowns are physically meaningful.

One may adopt a number of approaches. The pressure-field could be meaned temporally, so that for a given point p_0 could be taken as the five-day, monthly or seasonal mean. This method may not always be applicable synoptically, and care must be exercised when the general flow-field is changing rapidly, but by and large it works perfectly satisfactorily. Tests have shown that two analysts agree reasonably closely on p_0 under normal circumstances. In the case of an intense disturbance, say a low with a central pressure of 983 mb only a 3 per cent. error is committed by taking a " normal " pressure of 1,012 mb instead of 1,013 mb.

With regard to the measurement of R, this parameter gives the horizontal extent of the cross-section of a vortex at a specified level. There is no basis to Mr. Knighting's assertion that the proper parameter to take is $R(z)_{max}$. R may certainly vary with height,

but if the variation is marked, synopticians would surely have noticed before now that the horizontal scale of vortices changes with level. The simple assumption that R is constant with height seems to accord better with synoptic experience and we adopt this assumption tentatively, although there is nothing in vortex theory to prevent our treating $R(z)$ as variable.

If h and R cannot be used to specify the properties of an atmospheric vortex, what parameters can? The best, and only really valid critique of my paper is to produce a demonstrably better parametric representation of vortex structure.

If the relevance of my parameters is denied, and nothing better is substituted in their place, the critic will have to adopt the position that parametric representation is itself impossible, *i.e.*, he must deny that vortices exist and have a broad structure. It is quite a tenable position to affirm that the only reality is the contour-field, complete in itself and un-analysable, but it is hardly a standpoint from which one might anticipate striking progress. For better or worse, practical meteorology is wedded to the conception of vortices as things-in-themselves, separable, at least in theory, from the general flow-field, and the conception of a vortex as an entity implies the idea of structure.

It is true that we encounter mathematical difficulties when we try to split up a contour-field into a general flow and one or more vortices, but this is not a critique of vortex theory itself.

I should like to take this opportunity of amending estimates of cyclone parameters given in Table I of my paper. There the total potential energy was quoted on the assumption that the equivalent height was 6 km.

It has been found possible to estimate the mean equivalent height of occluded cyclones, and it is hoped soon to publish the results. The estimate of Z found for occluded cyclones was 11 km, so that the potential energies shown in column 9 of Table I should all be increased by 83 per cent., making the mean P.E. $25{\cdot}3 \times 10^{26}$ ergs.

The mean equivalent kinetic height was found to be 17 km, so that the total kinetic energy and virtual mass columns should be increased by 182 per cent., giving a mean (geostrophic) kinetic energy of $3{\cdot}6 \times 10^{26}$ ergs and a mean virtual mass of $2{\cdot}7 \times 10^{18}$ gm. The ratio of kinetic to potential energy now works out at 0·14 instead of 0·08.

R. W. James

C.S.I.R.O.,
Melbourne, Australia.
25 November 1950

Proceedings at meetings of the Society

Discussion Meeting, 18 October 1950

A joint discussion meeting with the Scientific Film Association was held on "The use of films in meteorology."

Short contributions were read by :

ROYAL METEOROLOGICAL SOCIETY. Professor Sir David Brunt, F.R.S., on "Application of films to meteorological research" ; Dr. J. S. Farquharson, on "Making a meteorological film" ; Mr. P. J. Meade, on "Application of films to training in meteorology."

SCIENTIFIC FILM ASSOCIATION. Mr. D. Segaller, on "Technical aspects of film making" ; Mr. J. D. Chambers, on "Factors to be considered in producing a film."

MINISTRY OF EDUCATION. Mr. C. R. W. Jenkins, H.M.I., on "Visual aids in teaching elementary meteorology in schools."

The following also took part in the discussion ; Instr. Cdr. J. A. Burnett, R.N., Mr. F. Gee, and Mr. J. Paton (*communicated*).

The following meteorological films were then displayed :

"Spotlight on Weather" (Twentieth Century Fox Films)

"Seeding of clouds" (Dr. V. J. Schaefer, G.E.C., U.S.A.)

"Flying the Weather Map." (American U.S. Navy, Bureau of Aeronautics, Training Section).

Ordinary Meeting, 22 November 1950

The election of the following Candidates was confirmed :—

Fellows

Appleby, John, 6 Penywain Road, Roath, Cardiff, Glam.
Aubrey, Guy Alban Beresford, British West African Meteorological Service, Bathurst, Gambia.
Blackwell, Michael James, 41 Beaumont Avenue, St. Albans, Herts.
Bradley, William Manser Frederick, 11 Lower Grove Road, Havant.
Brodie, Thomas Cooper, 3 Dugald Stewart Avenue, Bo'ness, West Lothian.
Cole, William John, "Cote," 3 Hill Burn, Henleaze, Bristol.
Davies, Ralph Llewellyn 21 Hawthorne Avenue, Uplands, Swansea.
Davis, Walter Ralph, "Tarramina," Southwood Road, Hayling Island, Hants.
Gifford, Thomas Henry Francis, "Wayside," 5 Scotch George Lane, Knaresborough, Yorkshire.

Griffiths, Cyril, 16 Snedden Street, Merewether, Newcastle, N.S.W., Australia.
Harnett, William, 85 Ballymun Road, Glasnevin, Dublin.
Hayes, John Keith, c/o 102 Alliance Road, Plumstead, London, S.E.18.
Hoyle, Richard, Fir Tree Farm, Hilton, Yarm, Yorkshire.
Jones, Cecil Harold, " Fairview," Sauncey Avenue, Harpenden, Herts.
King, Michael B., B.A., " Shepherd's Well," Berry Lane, Chorley Wood, Herts.
Paxman, Fg. Off. Denis J., B.A., R.A.F.V.R., 49 Olga Road, Dorchester, Dorset.
Ryder, Thomas Arthur, B.Sc., Ph.D., F.G.S., Cam Vicarage, Dursley, Glos.
Stewart, William Laing, 280 Ashley Down Road, Bristol, 7.
Tanner, Wallace Edward, 41 Andover Road, Orpington, Kent.
Tate, John Woodward, 4 Mission Row, Calcutta, India.
Vellacott, Edward, Broomhill, Purfleet, Essex.
Waddington, Denys A., " Clarendon," London Road, Marlborough, Wilts.
Watts, Captain Eric Ronald, " Red Roofs," Sandridgebury Lane, St. Albans, Herts.

Foreign Members

Bergeron, Professor Tor Harold Percival, Ph.D., University of Sweden, Uppsala, Sweden.
Bundgaard, Major Robert C., U.S.A.F., Evaluation Section, Directorate of Scientific Service, Air Weather Service, Andrews Air Force Base, Washington 25, D.C., U.S.A.
Feteris, Pieter Jacobus, 187 Sumatra Street, Leiden, Holland.

Associates

Arcand, Roger, Aviation Forecast Office, R.C.A.F. Station, St. Hubert, P.Q.
Barlow, F./O. James Leslie, R.C.A.F. Station, Greenwood, Nova Scotia.
Batchelor, F./L. Alexander William, R.C.A.F. Station, Summerside, P.E.I.
Carey, F./O. Gerald Harold, R.C.A.F. Station, Greenwood, Nova Scotia.
Clark, F./O. Robert John, R.C.A.F. Station, Summerside, P.E.I.
Dagg, Sqdn./Ldr. Ainsley George, R.C.A.F. Station, Greenwood, Nova Scotia.
Gordon, Sqdn./Ldr. Reginald Jack, R.C.A.F. Station, Greenwood, Nova Scotia.
Greenaway, F./L. Keith R., 557 Cole Avenue, Westboro, Ottawa, Ontario.
Holmes, Wg./Cdr. Arhtur Ray, R.C.A.F. Station, Centralia, Ontario.
Inglis, F./O. Grahame, R.C.A.F. Station, Summerside, P.E.I.
Jewett, F./L. Alden Lloyd, R.C.A.F. Station, Greenwood, Nova Scotia.
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The following papers were read and discussed :—

(a) Notes on the measurement and estimation of atmospheric radiation, by G. D. ROBINSON (*Q.J.*, **75**, p. 37).

(b) Evaporation over the British Isles, by H. L. PENMAN, (*Q.J.*, **75**, p. 372).

Reviews

Methods of Mathematical Physics. By H. and B. S. Jeffreys. Cambridge (C.U.P.). 2nd Edition, 1950. 4to. Pp. xii, 708. Four Guineas.

A well-known algebraist once said "all mathematics is applied mathematics." Whether this be agreed or not, it will probably be admitted that the theorems of pure mathematics which retain their place among the most important are those having a fairly direct bearing on applied mathematics. *Methods of Mathematical Physics* carries on the best tradition of Cambridge Natural Philosophy and will be placed alongside the works of such authors as Thompson and Tait, Rayleigh, Routh, and Lamb; but its chief distinction is that it contains much more pure mathematics. Although no theorems of pure mathematics are discussed that are not adequately treated elsewhere the approach to them is new. Their difficulties and importance are revealed through the eyes of the student of natural philosophy — or *mathematical physics* as it is now fashionable to call it. They are not presented as logical deductions from certain stated premises but as necessary objects of investigation before the technique for dealing with real problems can be set up.

It is refreshing to read this treatment of ideas hitherto regarded as in the field of the pure mathematician. Everyone knows that pure mathematics is a logical structure founded upon arbitrary definitions or axioms, but there is no need to present it as such and the authors have shown how each bit of theory has been developed for its usefulness as a necessary basis for applied mathematics. This does not mean that the proofs given are intended only to satisfy applied mathematicians for they have passed under the critical eyes of several of Cambridge's best pure mathematicians. The advantage of having the theory so presented is that one does not have to search for the relevant sections in a treatise on pure mathematics in order to ensure that one has not misused or misunderstood it.

The whole field of mathematical physics is covered by taking the methods as the primary objects of study and then making excursions into atomic physics, seismology, hydrodynamics and so forth, in order to illustrate the method. All these examples may be found by reference to the index, and most readers will find that it is by the index that one becomes introduced to the book. First one will seek out the treatment of a particular problem, and then become intersted in other applications of the method and later one will relearn with enthusiasm a great deal of pure mathematics in earlier chapters. Throughout the book the reader will find most stimulating comments of a philosophical nature.

If any theme could be said to run through a book of such wide scope it is the way of approaching problems. Equations are not sacred, in fact in every problem of physics the equality sign is abandoned at some stage and is replaced by an approximation or a mere inequality. To know how and when to make those replacements is the real problem. Most books on mathematical physics seem to assume that guidance in this matter is either unnecessary or impossible, but here it is given by a wealth of examples from all branches of physics. It requires boldness and imagination and one can at least learn the former by following these illustrations. As early as page 8 we find "Nature does not consist entirely, or even largely, of problems designed by a Grand Examiner to come out neatly in finite terms, and whatever subject we tackle the first need is to overcome timidity about approximating." Mathematical theory is made the servant and not the master. One is under no obligation to set up a model to which some standard piece of analysis or mathematical technique can be applied exactly, though it is the attempt to do this that spoils a great many recent studies in dynamical meteorology. The best approximation to reality is not always obtained by choosing the nearest model that gives an exact solution.

The first four chapters cover the real variable, scalars and vectors, tensors, and matrices. The axioms and definitions on which mathematics are founded are discussed in a way which reveals how they were chosen. The origin of difficulties which are glossed over in many textbooks is explained. For instance, let anyone who thinks he can derive the formula for the derivative of a vector referred to moving axes, read section 3.12, and then, as an exercise, derive the geostrophic wind equation. Chapters 5 and 6 deal with multiple integrals and potential theory, and are mainly straightforward but the notation is more sensible than in most treatments. The vector operator ∇ appears only in the form ∇^2; for the rest the suffix summation convention is used. In later chapters we find the familiar grad, div, and curl.

In the next two chapters we find the operational method for solving differential equations, and its applications. Chapter 10 is on the calculus of variations and discusses among other things Lagrange's equations and Hamilton's principle. Chapters 11 to 13 cover complex variable theory, contour integration, and conformal representation. Chapter 14 is the place for the student to learn the difficulties connected with Fourier series and Fourier's integral theorem after, and not before he thinks he has learned to use them. Here we find Professor Jeffreys's classical treatment of convection cells. The next chapter deals with the factorial function; let it be hoped that the world will follow the author and drop the *gamma* notation. Then follows a chapter on the formal solution of second order linear differential equations. Chapters 18, 19, and 20 discuss partial differential equations of mathematical

ysics — heat conduction, waves, gravitation, electrostatics, *etc.*, and the last five chapters describe ssel functions and their applications, the confluent hypergeometric function, Legendre and associated nctions, and elliptic functions ; and in all cases the book is good both as an introduction to them and reference.

Every chapter is infused with the authors' enthusiasm for the subject and is sprinkled with their ilosophy. Two chapters have not yet been mentioned for they have been reserved for special com- ent. Chapter 9 on Numerical Methods is a collection of the most useful and well tried devices. In ty pages we have just the treatment required by one who wants to use the methods. Textbooks voted entirely to the subject are too long and give a choice of too many methods to be much help work where the methods are incidental. Here we have methods chosen by authors who have con- derable experience in their use. Meteorologists will find it hard to agree that "graphical methods e best avoided entirely." They will argue that the accuracy required is so much less than in other bjects, but this is usually an attempt to rationalise a disinclination to learn numerical methods. nd numbers are so much easier on the eyes ! In any case the equations of meteorology are often ch that a numerical approach is the only approach. The development of modern computers will en up new lines of research, but such research must begin with ordinary desk computation. The atement that Gauss's integration formula is " best regarded as a museum piece " no longer applies r it has been found appropriate for automatic machines, and it is a pity that there is no discussion integration of differential equations by the Runge-Kutta method, for that too is most suitable for them.

The other, Chapter 17, entitled Asymptotic Expansions is characteristically Jeffreysian. It des- ibes the methods of stationary phase and steepest descent, the Airy integral, dispersion and group locity, and asymptotic solutions of differential equations. It should prove of great value to dynamical eteorologists, for the discussion of water waves contains many arguments applicable to gravity waves the atmosphere.

Mention must be made of the appendix on notation, for there is a tendency in meteorological riting to use symbols indiscriminately. The growing habit of starting a paper with a vocabulary of e notation is making authors too lazy to match their notation to established usage as nearly as possible. his appendix will give some guidance on what usage to follow, but what is more important, it will ake authors think more carefully about any new symbol it may be necessary to introduce. One may pe, however, that the remarks on p. 104 that D/Dt can always be replaced by d/dt will not be taken o seriously. It is true enough, but D/Dt is extremely useful in meteorology and is a great aid to larity of statement. Furthermore neither the symbol nor anything like it is required elsewhere.

Finally, the price — four guineas. The first edition was three guineas. It was not reviewed in is journal and although some changes have been made in the second they do not warrant comment this review. The paper is of improved quality but that cannot account for much increase and it difficult to understand how the changes in the text can have added so substantially to the price hen the book was produced from scratch for three guineas. But even granting that the wholesale rice increase is justified — and one will not grudge the authors and printers their share for both ave produced a work of the highest competence — what of the claims of the retailer ? Through the evitability of trade practices he receives his proportion of the extra guinea out of the blue ! He must ake an extra outlay but he is mainly paid for his services as a retailer. The ordinary investor is lucky he gets one quarter of the retailer's return on his capital. This book is no risk, but a certain sale, nd will remain a standard work for many years. Advanced textbooks are part of the lifeblood of niversities ; the authors do not write them primarily for a living, and students need them as part of eir equipment. The likely buyers belong by no means to a wealthy class and a jump in price of one uinea is bound to cause concern. Perhaps a consumers' council will find an answer ; after all it is he whole community which ultimately benefits from the dissemination of such literature to those ho can use it ; could not some way be found of distributing it in certain circumstances through braries or some other agency to students and others of meagre resources ? There is precedent, for tudents can obtain *The Times* at a reduced rate.

R.S.S.

The Atmospheres of the Earth and Planets. Edited by Gerard P. Kuiper, with contributions by 18 authors. Chicago, Illinois (University of Chicago Press), 1948. 8vo. Pp. vii, 366 ; 17 plates, 91 figures. $7·50.

This is a most fascinating and enjoyable book. It contains 17 distinct contributions by 18 authors, ll of which were read at the 50th Anniversary Symposium of the Yerkes Observatory in September 947. These papers have been collected together and edited by Gerard P. Kuiper, Director of the MacDonald and Yerkes Observatories and the Professor of Astronomy at Chicago University. As may e expected from a book prepared in this manner the individual chapters are disconnected and they nainly describe the contributions of the individual workers to this field. An attempt has been made

by the Editor in the first chapter to provide a thread which will bind the subsequent chapters together. This is however an almost hopeless task and the result is to provide an *hors d'oeuvre* rather than a logical connection. All the individual authors have tried to look at their work in the light of its general applicability to planetary physics and this more than anything provides the flimsy bridges which connect the chapters together.

The Editor was undoubtedly correct in avoiding the temptation to make a text-book out of his material. If he had done so we should have been denied the pleasure of having this book for two or three more years, there would have been a great deal of overlap with Mitra's book *The upper atmosphere*, and the refreshing enthusiastic style of active research workers with their plans and speculations about future work would have been lost. As it is we have a collection of research gems, and an example of how meteorologists, astronomers, spectroscopists, geologists, chemists, mathematicians and engineers can successfully apply their methods to the same problem.

The majority of the articles deal with the earth's atmosphere, and owing to the terms of reference given to the contributors these are mainly concerned with the upper atmosphere. It is a chastening thought that only two of the articles overlap at all seriously with Mitra's book, printed only two years earlier. Owing to the nature of the problem, optical and spectroscopic techniques are well to the fore and on account of the recent developments in infra-red detectors several contributions are largely concerned with this region of the spectrum. There is throughout the book, and rightly so, a strong emphasis on experimental method.

To give an idea of the scope of this book it would be necessary to go briefly through the contents, but although this is impossible it is worth while mentioning some of the contributions which will be of particular interest to readers of the *Quarterly Journal*.

C-G. Rossby, in his well-known lucid and critical style, has contributed *On the nature of the general circulation of the lower atmosphere*, and he describes the work of the Chicago group of meterologists with particular reference to the " vorticity transfer " theory and its application to the terrestrial and solar atmospheres. H. C. van de Hulst has made a real contribution to meteorological and astrophysical literature with his critical survey of *Scattering in the atmospheres of the earth and the planets* although unfortunately he deals rather casually with the important problem of the earth's albedo.

J. L. Greenstein and others give us the latest position upon *The upper atmosphere from rockets*. This work has yet to suffer the soothing influence of time, and should be read mainly as a progress report with some mental reservations. As an example of the difficulties involved, H. E. Newell (1950 *Trans. Amer. Geophys. Union*, **31**, p. 25.) finds that the double maximum of ozone concentration shown in Fig. 47 is fictitious, and indeed it is made to appear both from this book and the original papers that the existence of two maxima and a minimum of ozone concentration was based mainly upon four measurements. In a book of this nature we might hope for a more detailed and mature analysis of errors than is given. Errors in temperature measurement are given as $\pm$ 25°C, but Newell's paper gives the impression that this is an underestimate.

A most interesting contrast to these direct measurements is provided by F. L. Whipple and coworkers who describe new results upon the density of the upper atmosphere obtained from meteorite observations. The history of the earth's atmosphere is treated from geophysical and astrophysical evidence by R. T. Chamberlin and H. Brown respectively, who manage to achieve completely different conclusions. Finally a series of articles largely on the application of infra-red spectroscopy to terrestrial and planetary problems are contributed by A. Adel, M. V. Migeotte, T. Dunham, G. P. Kuiper and G. Herzberg. In amongst a galaxy of brilliant researches, Herzberg's photographs of the quadripole vibration-rotation spectrum of hydrogen are still remarkable.

A book which does not pretend to have much form or continuity cannot easily be criticised, but it is relevant to remark that the title is misleading and that the addition of some such words as " *Some recent advances in our knowledge of* . . . . . " would make it less spectacular but more truthful. There is no space here even to mention the very many other researches, largely performed in the United States, which are not mentioned in this book, but there is one omission which comes near to destroying completely such balance as the book does possess. Could we not have been given a chapter describing some of the many recent advances which have been made in our knowledge of the ionosphere ? The Editor apologises for this omission in his Introduction and devotes a page or two to the subject, but this serves to emphasise rather than to repair the damage.

R.M.G.

The Flight of Thunderbolts. By B. F. J. Schonland, F.R.S. Oxford (O.U.P.), 1950. 8vo. Pp. viii, 152 ; 33 figures. 15*s*.

The central theme of this book is a clear and non-technical account of present knowledge of the processes and mechanism of the lightning discharge. In the attainment of this knowledge the team of South African workers, under the brilliant direction of Dr. Schonland, has of course made the major

ntribution. The author also discusses, at some length, the problem of protection of life and property ainst lightning and takes a passing glance at the complex question of the mechanism of electrification thunderstorms.

In his enthusiasm for a subject of peculiar fascination the author has combined with his central eme a catholic assortment of topics, dealing with primitive superstitions and, at considerable length, ith the historical development of ideas about lightning and protection therefrom, a subject abounding curious by-ways. Dr. Schonland casts his net very wide ; in the index Frenkel rubs shoulders with Franklinismus " and Pallas Athene with " Partial discharges," (not to mention Wormell with Zeus). here has resulted an unusual work which successfully combines entertainment with instruction ; first-rate bedside book.

T.W.W.

lements of applied hydrology. By D. Johnstone and W. P. Cross. New York (Ronald Press), 1949. 8vo. Pp. x, 276 ; 97 figs., $5.

This book, well written and well illustrated, is for the use of American undergraduate students civil engineering. It gives a general account of principles involved in three main problems of stream ow : mean annual flow ; drought flow ; and flood flow. Students (under an instructor) are expected apply these principles to data collected by them from their own local watersheds, and, for uidance, the last four pages of the book provide a list of problems, questions and field exercises. he idea is admirable, and given an enthusiastic instructor the students using the book will get a ery good grounding in the field side of hydrology.

H.L.P.

Upper winds over the world. Geophys. Mem., No. 85. By C. E. P. Brooks, C. S. Durst, N. Carruthers, D. Dewar and J. S. Sawyer. London (H.M.S.O.), 1950. 8vo. Pp. 150 ; 19 figures, charts 23 tables. 17*s.* 6*d.*

The main stimulus to the production of the Memoir under review arose from the practical needs f air travel. The economic operation of air lines and even the safety of aircraft depends on an accurate nowledge both of the average winds and of the statistical distribution of variation from the mean seasonal) value. For if this knowledge is combined to give the mean value and variation of the effective head-wind the maximum safe pay-load of a given aircraft on a given route can be computed. There re two distinct problems (or sets of problems) to be solved :

(1) to determine as accurately as possible the mean wind-field and the manner and extent of its variation

(2) to present the data in a form suitable for the estimation of equivalent head-winds.

The first is a basic problem in climatology (which if it is to be a science must be concerned with he upper air no less than with surface phenomena) and therefore of general interest. The second is nore specialised and can be discussed more briefly.

Let us consider first what might appear to be the simplest problem, that of the mean (seasonal) vind-field. Previous work, such as that of Sir Napier Shaw, has given us a fairly good idea of the shape nd spacing of isobars at high levels. But since the time such maps were produced a great deal of new nd more accurate information has become available so that improved versions might be constructed. This in fact has now been done in this Memoir, and in the more convenient form of contours of isobaric surfaces up to 130 mb for four seasons of the year. It is assumed that the geostrophic approximation may be used when measuring up these charts in middle and high latitudes and this assumption s probably justified when account is taken of the probable error involved as compared with errors ssociated with inadequacy of the network and shortness of period of the data, especially of direct vind measurements. In low latitudes the method of measuring up contours fails, not so much from he failure of the geostrophic approximation as applied to the *mean* contours but because it is impossible o determine the mean contours from temperature measurements with sufficient accuracy — a point ot made clear in the Memoir. We are therefore given streamlines and isopleths of speed derived from study of pilot-balloon ascents. Fortunately the authors do not take the measurements at their facevalue. Simple means give too small a value for high-level winds because of a tendency for early loss of balloons in strong winds. Loss of balloons in cloud also leads to systematic errors, as is indicated by comparison with nephoscope reports or, where available, radar winds. The authors are to be congratulated on the care they have taken in attempting to " correct " for these errors. While their results are necessarily to some extent subjective there is no doubt that they are nearer the truth than any conceivable purely *objective* method.

While that part concerned with the mean field may be regarded merely as refinement of existing knowledge the part concerned with the variation of the wind field breaks new ground. The authors

have discovered that it is unnecessary to construct sets of wind roses, for the variation of the wind at any point may be represented with reasonable accuracy by a *single* variance parameter. Given this parameter the *magnitude* of the vector deviation is statistically distributed according to the normal (Gaussian) law and all directions are equally probable. While not entirely unexpected this result is of considerable theoretical interest, and further investigations regarding the accuracy of the law are desirable. Even so the production of charts of the variance parameter is more difficult than that of charts of the mean field. In searching for some sort of guide to extrapolation in " blank " areas the authors hit on a second, this time *purely* empirical, " law " describing the variation of the variance σ with height or, more precisely, density. Apparently we have, over most of the troposphere above any place, $\sigma\rho$ = constant, approximately. While the general form of this relation (with maximum variance fairly near but definitely below the tropopause) is intelligible theoretically there is no strong evidence either theoretical or empirical for its exact form and it must be borne in mind that the charts over wide areas are to some extent suspect in so far as they have been constructed by analogy using this relation. On the other hand one striking feature of the charts (which may come as a shock to some people), the large maximum of variance in low latitudes just below the tropopause, is consistent with recent American studies. It may be noted that, as a by-product of their investigation, the authors have evaluated a quantity of fundamental importance in physical climatology, for σ^2 is a measure of the total (principally large-scale) *turbulent energy*.

For the application of these statistics to flight problems it will suffice to refer to a very lucid account by J. S. Sawyer *Equivalent Headwinds* (*Met. Office*, London, Met. Reports, No. 6).

E.T.E.

Recent additions to library

H. J. Allcock and J. Reginald Jones, *The Nomogram*, the theory and practical construction of computation charts, 4th edition, Revised by J. G. L. Michel. Pitman, 1950. Pp. x, 238, 81 figs. 18*s*.

Atmospheres of the Earth and Planets. Papers presented at the Fiftieth Anniversary Symposium of Yerkes Observatory, September, 1947. Edited by Gerard P. Kuiper. Chicago, Illinois, University of Chicago Press, 1948. Pp. vii, 366, 17 plates, 91 figs. $7.50

C. E. P. Brooks, *Climate in everyday life*. Ernest Benn, 1950. Pp. 314, 34 figs. 21*s*.

V. Conrad and L. W. Pollak, *Methods in climatology*. Cambridge, Massachusetts, Harvard University Press, 1950. Pp. xxvii, 459, 115 figs., 101 tabs. $7.50.

Karl Keil, *Handworterbuch der Meteorologie*. Frankfurt A.M., Fritz Knapp, 1950. Pp. vii, 604 illus. 30 Marks.

Ed. Le Danois, *Le rhythme des climats dans l'histoire de la terre et de l'humanité*. Paris, Payot, 1950. Pp. 204, ii, 18 figs., 480 fr.

Monthly meteorological charts of the Eastern Pacific Ocean. Prepared in the Marine Branch of the Meteorological Office, Air Ministry, London, M.O. 518 (formerly M.O.M. 446). London, His Majesty's Stationery Office, 1950. Pp. 122, 25 charts. £4 4*s*.

Monthly sea surface temperatures of North Atlantic Ocean. Marine Branch of the Meteorological Office, Air Ministry. London, His Majesty's Stationery Office, 1949. Pp. 14, 12 maps. 10*s*.

B. F. J. Schonland, *The flight of thunderbolts*. Oxford University Press, 1950. Pp. viii, 152, 6 plates, 33 figs. 15*s*.

The Thunderstorm. Report of the Thunderstorm Project (a joint project of the four U.S. Government Agencies : Air Force, Navy, National Advisory Committee for Aeronautics, and Weather Bureau). Washington, D.C., Government Printing Office, June 1949. Pp. ix, 287, 254 figs., 49 tables. $2.25.

Transactions of Oslo Meeting August 1948, *IATME* Bulletin No. 13. Washington, International Union of Geodesy and Geophysics, Association of Terrestrial Magnetism and Electricity, 1950. Pp. xiv, 568, illus. No price given.

Upper winds over the world. *Geophys. Mem.*, No. 85. By C. E. P. Brooks, C. S. Durst, N. Carruthers, D. Dewar and J. S. Sawyer. London (H.M.S.O.), 1950. 8vo. Pp. 150 ; 19 figures, charts, 23 tables. 17*s*. 6*d*.

Sea atlas round Australia, Oceanographic and meteorological data, K.N.M.I. No. 124, De Bilt, Royal Netherlands Meteorological Institute, 1949. Pp. 79, charts, tables. F20.

Geochimica et Cosmochimica Acta. Butterworth-Springer, 1950. Vol. 1, No. 1. £3 10*s*. per volume.

Obituary

Professor E. A. MILNE, M.B.E., D.Sc., F.R.S.

Edward Arthur Milne, first Rouse Ball Professor of Mathematics in the University of Oxford, d suddenly in a Dublin hospital on 21 September 1950 at the age of 54. He had been in poor lth for some time and was taken ill while on his way to attend a colloquium convened by the Royal ronomical Society in the rooms of the Royal Irish Academy, where he was to have read an important v paper on a connexion between gravitation and magnetism indicated by recent developments inematic relativity. Eldest son of the head master of an elementary school at Hull, Milne received early education there and at Hymers College in the same city, whence he proceeded to Trinity llege, Cambridge as a mathematical scholar in 1914. Two years later his undergraduate career s interrupted by the call of the 1914-18 war. He joined the Anti-Aircraft Experimental Section, nitions Inventions Department, at Portsmouth. With the award of the M.B.E. for his contri- ions to the national effort he returned to Cambridge in 1919 and took a war degree. Before the se of that year the high intellectual endowments which he had displayed while under tuition from 4 to 1916 and in his work at Portsmouth gained him Fellowship of Trinity College. In 1920 lne was appointed assistant director of the Solar Physics Observatory at Cambridge under Prof. F. Newall, and four years later he succeeded Sydney Chapman as Beyer professor of applied thematics in the University of Manchester, a post which he held until, in 1928, he was elected the then newly-founded Rouse Ball Chair of Mathematics at Oxford, with Fellowship of Wadham llege. Here he passed the rest of his life, apart from a period of research on ballistics for the dnance Board at Chislehurst during the 1939-45 war.

Outstanding brilliance and originality of mind combined with deep scientific insight and seemingly undless mental energy put Milne in the front rank of mathematical physicists and enabled him break new ground in a wide variety of fields, including astrophysics, cosmology and relativity. s direct contacts with meteorology came at an early stage in his career, when branches of the subject nged on a theory of stellar structure which was his main concern at that time. In 1920 he colla- rated with Prof. Sydney Chapman in presenting to this Society an important paper entitled "The mposition, ionisation and viscosity of the atmosphere at great heights" (*Q.J.*, **46**, Pp. 357-396). n years later a note of his own on "The convective equilibrium of an atmosphere" appeared in l. 56 of the *Journal* (p. 405). He was elected a Fellow of our Society on 15 November 1933. By en, however, major problems of the universe were urging their permanent claim on his powerful ellect and he made, regrettably, no further contribution to meteorological literature. Among ilne's many academic honours were Fellowship of the Royal Society (1926), the Gold Medal of e Royal Astronomical Society (1935), a Royal Medal of the Royal Society (1941), and the Bruce edal of the Astronomical Society of the Pacific (1945). He was president of the London Mathe- atical Society in 1937-39, and president of the Royal Astronomical Society in 1943-45. High urage supported by unswerving religious convictions gave him the mental strength to overcome ysical disabilities and tribulations of private life to which few men could so triumphantly have en superior.

E.L.H.

Captain H. S. MURRAY-SMITH, R.N.

Captain Harry Stewart Murray-Smith, whose death after a long and courageously-borne illness curred on 3 June 1950, his 53rd birthday, was a pioneer of naval flying and one of the first to qualify a naval air observer. The potentialities of combined operations by sea and air became his main terest on entering the Royal Navy in 1915, and to the successful development of this branch of odern warfare by British forces he made important contributions in the course of a distinguished reer. During the Second World War he held for a time an appointment with the rank of commodore light fleet carriers operating in Pacific waters. At another period of the hostilities he was in command the Royal Naval Air Station at Yeovilton, Somerset. Later, his services in connexion with the lerno landings earned him a mention in despatches. When still a young man Murray-Smith alised the vital bearing of meteorology on aviation and took up the study of our science with en- usiasm. He was elected to fellowship of the Royal Meteorological Society on 17 November 1926 a lieutenant aboard H.M.S. *Hermes*. For much of the information contained in this notice we e indebted to the courtesy of his widow, Mrs. Anne Murray-Smith.

E.L.H.

Mr. A. G. MURDOCH

Alexander George Murdoch, of Gallichoilly, Lochgilphead, Argyllshire, who had been a Fellow this Society since 16 June 1948, was killed at the age of 29 in a flying accident over Scone Airfield, rthshire, on 15 September 1950. The Anson aeroplane which he was piloting collided with another

during a close-formation training flight undertaken in preparation for the Battle of Britain displa on the following day. Mr. Murdoch served in the R.A.F.V.R. from April 1942 to February 1947 In 1948 he entered the employment of Airwork Limited as flying instructor at Grangemouth, Stirling and on the closing down of the aerodrome there he was transferred to Scone. A director of Airwor Limited has described him as the company's most efficient pilot and as possessing both flying abilit and technical knowledge in exceptional measure. Mr. Murdoch held first and second class civi aircraft navigators' licences and was a member of the Guild of Air Pilots and Air Navigators of th British Empire.

E.L.H.

Mr. I. M. CRAWFORD, B.Sc.

Iain Main Crawford lost his life on 17 October 1950, when the Vampire jet fighter which h was piloting came into collision with another Vampire over Kirkburn, Yorkshire. Born at Ardrossan Ayrshire, in 1928, Mr. Crawford received his education at Dumfries Academy and the University of Edinburgh, where he took an honours B.Sc. degree in physics in 1949. An enthusiast for flyin since boyhood, he joined the Edinburgh University Air Squadron and obtained his civil licence during the course of three years' active membership therein. He entered the Royal Air Force on 28 October 1949 and was commissioned five days later. His interest in meteorology dated from his school-days, and while an undergraduate at the university he made a special study of the subject Mr. Crawford was elected a Fellow of this Society on 22 February 1950.

E.L.H.

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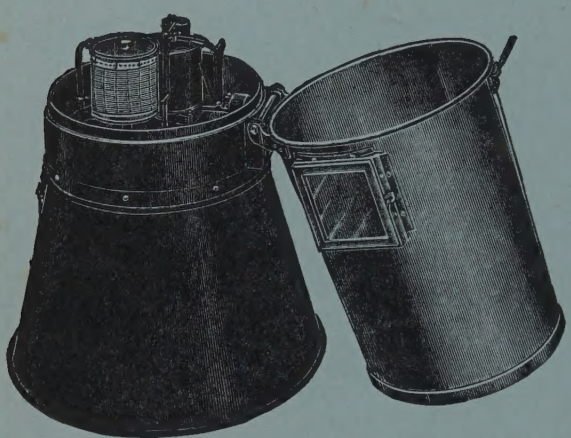

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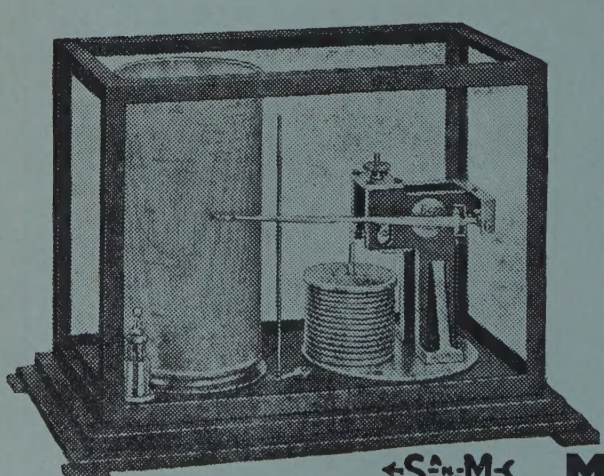

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